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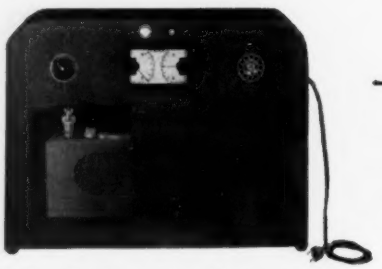

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Annual Meeting, AAAS, Cleveland, Ohio, December 26–30, 1950

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THE
SCIENTIFIC MONTHLY

OCTOBER 1950

A Primary Social Science Law

CLARK L. HULL*

The author took his Ph.D. in psychology at the University of Wisconsin in 1918, where he later became professor of experimental psychology. Since 1929 he has been research professor of psychology at the Institute of Human Relations of Yale, where his efforts have been largely directed to the formulation of equations representing the quantitative primary natural science laws of mammalian behavior and their systematic organization into secondary laws.

ONE hears much these days about the backwardness of the social sciences and the urgent need to hasten their development. In the opinion of many social scientists the hope for an early and useful development of these sciences depends upon three essential conditions: that all social phenomena occur according to invariable *primary* laws; that these primary social science laws are *quantitatively* determinable; and that they are statable by means of *true equations* of the type expressing the law of falling bodies.

This is to say, in short, that the social sciences are true natural sciences. Since this view of the nature of the social sciences is by no means universally accepted, a related sequence of concrete examples of a single law from this rich field will be presented as a kind of inductive demonstration of the general empirical soundness of the three conditions stated. Incidentally, the law we have selected for illustrative purposes will be seen to operate through a wide, and to some persons a surprising, range of phenomena, thus integrating into a single science facts which possibly may not have been previously thought of as being scientifically related.

* The author wishes to thank George P. Murdock for aid in preparing the material in the present manuscript which is concerned with cultural anthropology.

The primary social science law in question is that of stimulus generalization.[5, 6] It is significant of the wide range of its operation that it was first investigated experimentally, and then formulated, by I. P. Pavlov, the great Russian physiologist. He diverted some of the salivary ducts in dogs through the outer skin and was thus able to measure the amount of saliva secreted (Fig. 1). Using this quantitative methodology, Pavlov was able to report the preliminary observation of a social science or law:[11]

> . . . if a tactile stimulation of a definite circumscribed area of skin is made into a conditioned stimulus, tactile stimulation of other skin areas will also elicit some conditioned reaction, *the effect diminishing with increasing distance of these areas from the one from which the conditioned reflex was originally established* [italics ours].

Typical quantitative results of this sort were later published by G. V. Anrep.[1] The point on the dog's skin (the stimulus continuum) conditioned to the salivary reaction and the various additional points tested for stimulus generalization are shown in Figure 2. A quantitative graphic representation of the outcome of the experiment is given in Figure 3.

But how general is this supposed law? During the early days of the Institute of Human Relations, Bass and Hull[2] carried out an experiment to test the generality of the law, using the galvanic skin

reaction on young men and thereby changing the type of reaction and the type of subject as well. The stimulus arrangement used is shown in Figure 4. The outcome of this experiment was in practically exact agreement with Anrep's, in that the gradient is a falling one, as Pavlov had formulated and as may be seen in Figure 5. These results indicate that the law of stimulus generalization presumably operates in both dogs and humans, in the galvanic skin reaction and the salivary reaction. They therefore represent an appreciable degree of advance toward a behavioral integration.

Our next step in demonstrating the natural science nature of stimulus generalization is to show—in a preliminary way, it is true—that the galvanic skin reaction follows, in human subjects at least, a mathematical law as a function of the auditory pitch continuum. This experiment was performed by Carl I. Hovland (Fig. 6).[4] Because of Hovland's rather precise quantification of the scale separations in this experiment, it has been possible to fit what is believed to be a significant equation to his results. This is represented by the smooth curve running through the data points just above the statement of the equation shown in the figure. It will be noticed that the function does not fall to zero even if extrapolated, presumably because many of the stimuli to which the response is conditioned, such as the apparatus stimuli and those arising within the subject's body, remain constant, i.e., they do not change with the pitch of the pure tone which is deliberately changed.[6] Stimulus generalization thus emerges as a natural science quantitative law, in this respect quite like the primitive law of falling bodies.

So far all the evidence presented on stimulus generalization has concerned the conditioned reflex. The next step is to consider whether the stimulus generalization law holds for ordinary habit formation. This problem was attacked by J. S. Brown.[7] He trained groups of hungry albino rats to run down a straight alley toward a light at its end in order to obtain food. After this training had reached a fair degree of perfection, the rats were placed one at a time in a little harness made of rubber bands, and when they had reached the middle of the runway they were held back by a recording coil spring that indicated how strongly they would pull to get to the stimulus light, and so to the food at the end of the alley, the intensity of the test light often being varied widely and systematically from the trained intensity (Fig. 7). It is evident that the law of stimulus generalization operates here with a falling gradient, substantially as in the conditioned reflex. Thus, so far as this stimulus generalization law is concerned, the conditioned reflex in dogs and humans is integrated with ordinary habit in albino rats. As a result, one more very important integration is effected.

Proceeding further in our integrational investigation of the social or behavioral sciences, we meet the question of whether a law which operates in isolated organisms also operates in social science situations in the narrow sense, i.e., in situations where essentially more than one organism is involved. An elementary case in point is found where one organism becomes a critical stimulus for the behavior of another. Neal E. Miller and John Dollard[8] carried out a decisive yet simple experiment on this point, using two groups of hungry

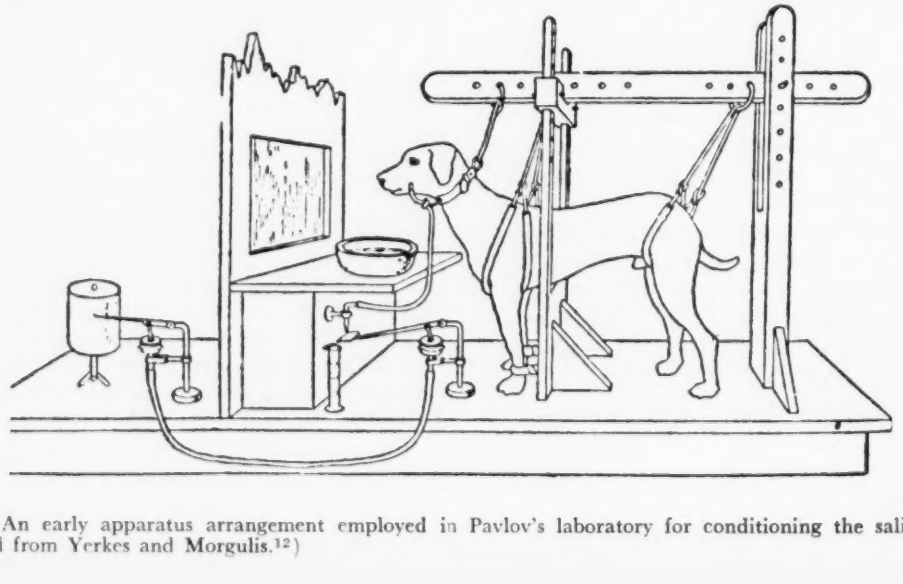

FIG. 1. An early apparatus arrangement employed in Pavlov's laboratory for conditioning the salivary reflex. (Reproduced from Yerkes and Morgulis.[12])

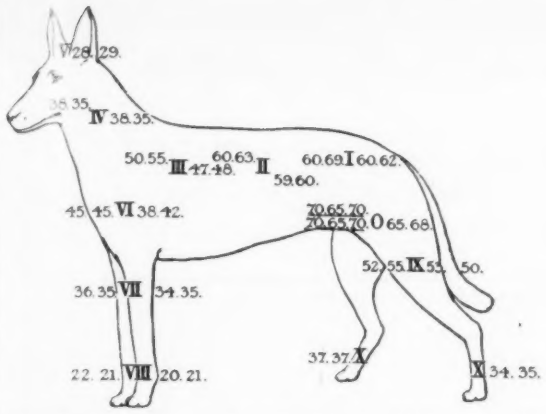

FIG. 2. The point on Anrep's dog at which the conditioned salivary reflex was set up (0), and numerous other points tested for stimulus generalization. Only points 0, II, VI, VII, and VIII are represented in Figure 3. (Reproduced from Anrep.[1])

albino rats. For convenience we shall call the first group "stimulus" animals, and the second group "response" animals. Miller and Dollard first trained the stimulus rats to run up the stem of the elevated T-maze shown in Figure 8, to cross the gap to the right or left, and then to go to a black or white card placed at one or the other arm of the T by chance arrangement, where a bit of commercial dog food was found and eaten. Naïve response rats of the same type were then placed on the maze, but behind the trained stimulus animals. Half of the new response animals were easily trained to turn in the *same* direction as the stimulus animals, and half were trained to turn in the *opposite* direction from that taken by the stimulus animals. At a point marked by the strings (shown about midway up each arm toward the card, Fig. 8), the response animals received dog-food reward when they had taken the correct path.

After twelve days of training, seven trials per day, the animals of the response group as a whole took the same turn as the stimulus animals (cards being absent) on 84 per cent of the fifty-six trials, whereas the "contra" group of the response animals took the same turn as the stimulus group only 15 per cent of the fifty-six trials, pure chance of course yielding 50 per cent. The difference between 84 and 15 is 69, which we shall take as an index of the composite strength of the two tendencies to respond to the behavior of the stimulus animals. This is represented by the column at the left in Figure 9.

At this point Miller and Dollard ingeniously introduced into this social situation a test for the law of stimulus generalization, by using black instead of white stimulus animals, a marked difference on the stimulus continuum. The outcome is shown by the column at the right in Figure 9. Quite as one would expect, there was still a distinct tendency to respond to the stimulus animals, even though they were black. The index of the turning tendency, moreover, has clearly fallen, according to the stimulus generalization law of Pavlov and the other investigators whose work we have considered. No doubt if a gray rat had been used as a stimulus animal, an index value a little less than the mean of the present values would have been secured. Once more we observe an extension of the scientific integration demonstrated by a common natural science law to include the responses of one organism to the stimulus presented by the behavior of another organism, i.e., to include *social* behavior in a very elementary form, probably allied to the extremely important phenomenon of imitation.

Social behavior ordinarily involves more than merely responding to another organism as a stimulus. For one thing, it frequently involves the *mutual* stimulation of at least two organisms, and mutual

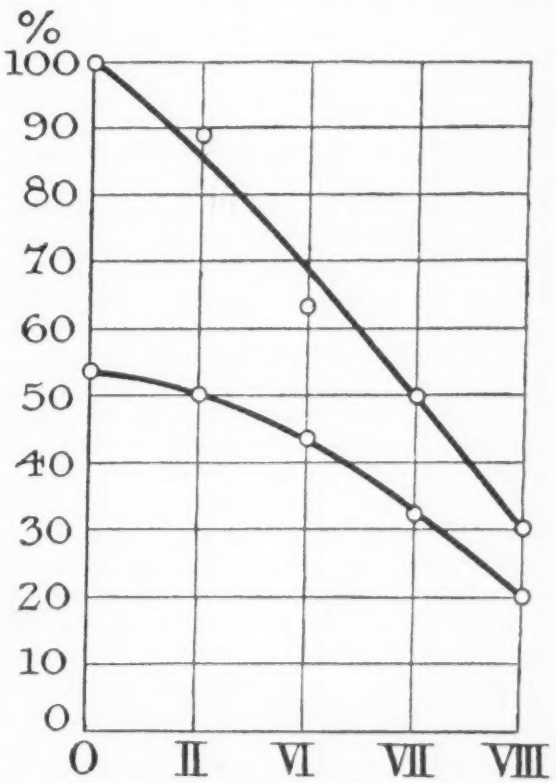

FIG. 3. Graphic representation of the generalization gradient of the salivary conditioned reflex reported by Anrep. The lower graph represents the same spots as the upper one at a later stage in the experiment. (Reproduced from Anrep.[1])

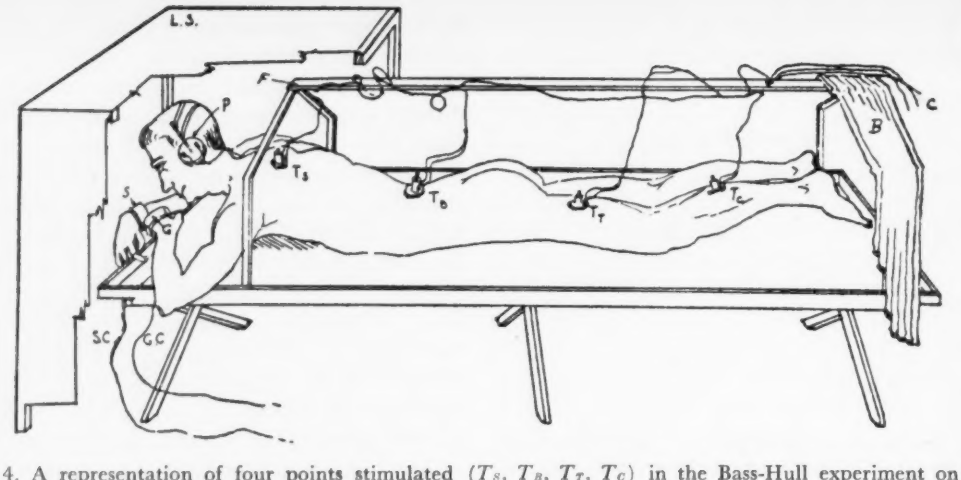

FIG. 4. A representation of four points stimulated (T_S, T_B, T_T, T_C) in the Bass-Hull experiment on stimulus generalization of the conditioned galvanic skin reaction on human subjects. (Reproduced from Bass and Hull.[2])

responses to those stimulations, the acts sometimes being directed to the body of the other organism. Miller[7] also investigated this more intimate type of social situation at a simple level, once again with albino rats. He placed these animals together by pairs on a charged electric grid and, when by chance they approached each other, he reinforced this act by terminating the shock. Later he did not turn off the shock until the rats moved their paws more or less as in fighting. Finally the rats of the entire group would strike each other vigorously, as in ordinary rat-fighting, at once after the shock was turned on (Fig. 10).

The operation of the law of stimulus generalization in this social interaction situation was demonstrated as follows. After the original training had taken place, two rats were put on the grid with a celluloid doll (Fig. 10), the visual appearance of a rat and that of the doll being well separated on the stimulus generalization continuum. In such cases, eleven out of twelve trained animals began striking each other as soon as the shock was turned on; only one animal struck the doll and this animal did so briefly, at once turning to strike the second animal. But when placed on the grid with the doll alone, as in Figure 11, six out of the twelve animals knocked the doll over by striking it with the paws, whereas the remaining six upset the doll by chance running against it. Twelve control animals not trained to strike another rat when shocked were put on the grid with the doll, one at a time. Only one struck at the doll, whereas the other eleven rats upset the doll by chance running against it. Miller concludes from these results that the trained attack behavior generalized from the other animal as a stimulus object to the doll was much weaker than from one stimulus animal to another. This is, of course, exactly in line with the law of stimulus generalization, including its falling gradient as seen in the preceding examples. Miller believes with reason that this experiment represents the substance of the human psychoanalytic phenomenon of displacement.[7] Thus the law of stimulus generalization is seen also to be operating in an elementary interactive social situation, which constitutes an additional integration of individual behavior to social behavior of a very special sort.

The law of stimulus generalization is known to apply to both the free motor and the symbolic social behavior of human beings, although no simple quantitative demonstration has come to hand. Young children notoriously generalize the response of calling the father "daddy" by applying the term to other men. They are apt to call the first rabbit they see a "kitty," and the first horse a "cow," and so on. Clinical evidence of stimulus generalization is also abundant. Both psychoanalysts and psychiatrists, for example, recognize that adult males in our own society characteristically react toward a boss or other person in an authoritative position with types of behavior acquired in childhood and adolescence toward the individual's own father.

With this observational transition we move forward into the field of cultural anthropology. Leading anthropologists assume that the "cultures," or bodies of custom, of human societies represent adaptive responses learned by collectivities of individuals over time. These cultures should, therefore, reflect all the factors involved in individual learning, including the phenomenon of stimulus generalization, and this should be susceptible of quantitative demonstration through comparative studies of cultural materials. We shall select one of a considerable number of examples given by George P.

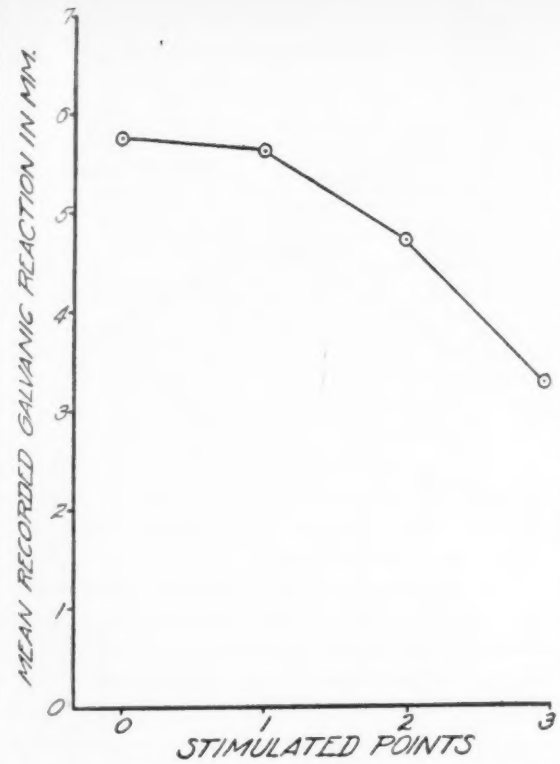

FIG. 5. Graph showing the gradient of stimulus generalization of the conditioned galvanic skin reaction as a function of distance of the generalization test points from the point conditioned (0) on the human skin as a stimulus continuum. (Reproduced from Bass and Hull.[2])

Murdock[9] with respect to the linguistic terms applied by different societies to various types of relatives as these relate to the cultural rules governing where a man and wife reside after marriage.

The four most common rules of residence are: *neolocal,* where, as with ourselves, a couple normally establishes a new household separate from the homes of the parents of both; *patrilocal,* where they live with or near the husband's parents; *matrilocal,* where they live with or near the wife's parents; and *bilocal,* where they live either patrilocally or matrilocally, depending upon economic or prestige factors. Murdock's evidence shows that there is a tendency for the term meaning "mother" in any language to be extended to aunts who normally live with or near the mother under the prevailing rule of residence. This is rarely the case with either neolocal or patrilocal residence. Under matrilocal residence, however, the maternal aunt resides in the same household as the mother or in a dwelling close by, and under the bilocal residence both paternal and maternal aunts are likely to be spatially associated with the mother; under these conditons the term "mother" tends to be applied to aunts.

Any aunt who resides with or near the mother is likely to assist the latter in caring for, playing with, disciplining, and even nursing her children, and thus to a considerable extent she plays the role of a secondary mother. Under bilocal residence, a growing child comes to regard both paternal and maternal aunts as people who resemble and act like his mother, and this condition prevails even after marriage if he remains at home.

Here enters the principle of stimulus generalization. The term used for "mother" is here regarded as a response. This response will naturally generalize to other relatives as a function of the similarity of the stimuli associated with them in relation to the stimuli provided by the individual's own mother. In all cultures there are some similarities; aunts as well as mothers, for example, are females. Where bilocal residence obtains, the stimulus similarities between aunts of either type and the actual mother approach identity, in contrast to other rules of residence, and this constitutes the basis in the stimulus continuum for the generalization gradient. Thus people should tend to call both paternal and ma-

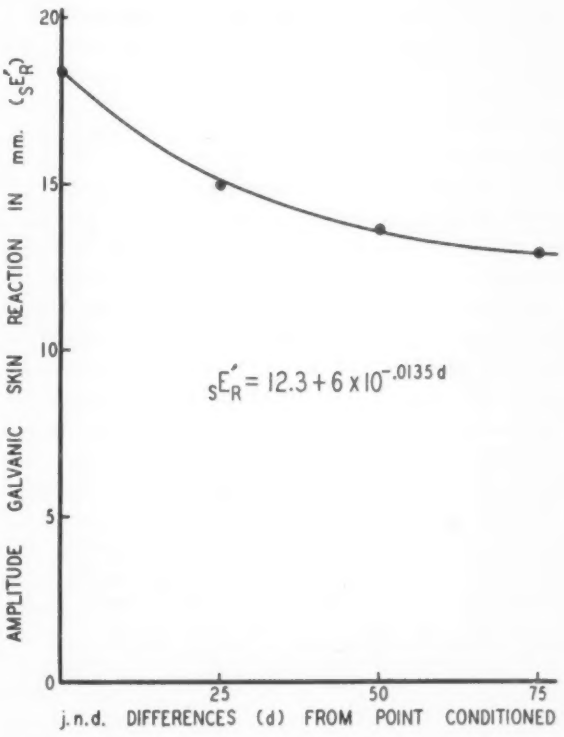

FIG. 6. Graph showing the four points determined by Hovland on his stimulus generalization gradient of the conditioned galvanic skin reaction with humans, based on pure tones tested at psychologically equal distances from the tone originally conditioned. The equation represents the curve fitted to the data points indicated by the circles. (Plotted from data published by Hovland.[4])

ternal aunts "mother" more strongly in cultures having bilocal residence than in those following other rules.

It must be observed here that the concrete manifestations of the stimulus generalization gradient in the situations considered previously are strikingly different from those found in this and certain other situations. The graphs in Figures 3, 5, 6, 7, and 9 show that as the stimulus continuum becomes increasingly different from the point of origin, the response being generalized becomes weaker and weaker, nothing in particular taking its place. But in a situation where there is a definite competition between two responses of the all-or-nothing variety, such as the custom of calling the aunt "mother" or some other name, a distinctly new element enters, namely, that of chance. Moreover, the probability of the respective responses in a given culture will depend not only upon the strength of the one being generalized but also upon the strength of the other or competing response tendency, together with large numbers of smaller but unknown factors. This means that a problem of this kind cannot be solved by observing a single culture, but must be based on a very large number of cultures. By the pooling of many cultures the influence of irrelevant factors tends to be equalized. It also means that when this large number of cultural data is assembled it will make up a fourfold, or tetrachoric, correlation table or surface.

It happens that for years anthropologists have been patiently studying the ways of great numbers of primitive peoples and publishing their findings. Recently the Yale Institute of Human Relations has been collecting and assembling these widely scattered studies systematically. This is a research project which was initiated by Murdock and known as the Cross Cultural Survey. (This project is now known as the Human Relations Files, Inc., and is directed by C. S. Ford.) A tiny sample is shown in Table 1. The coefficient of association (Q) used by Murdock is a simple one, depending primarily upon the fact that in a positive relationship the product of items $a \times d > b \times c$. For example, in Table 1, $a \times d = 1{,}683$, whereas $b \times c = 363$, which indicates that there is a definite tendency for bilocal residence to be differentially associated in cultures having the tendency to call the aunts "mother." In order to convert the tendency into a quantitative index ranging from zero to 1.00, the following simple formula is used:

$$Q = \frac{(a \times d) - (b \times c)}{(a \times d) + (b \times c)}.$$

Substituting in this formula, we have

TABLE 1

The number of cultures calling the maternal and paternal aunts "mother," and those not doing so, as a function of whether the custom of bilocal residence is in operation. (Adapted from Murdock, p. 152.[9])

Bilocal Residence Customs		Other Residence Customs	
Aunts called "mother" (a)	Aunts not called "mother" (b)	Aunts called "mother" (c)	Aunts not called "mother" (d)
9	11	33	187

$$Q = \frac{1{,}683 - 363}{1{,}683 + 363} = \frac{1{,}320}{2{,}046} = +.645.$$

Appropriate computations show that the value so obtained would be secured by chance only once in about one hundred times.[9]

It is evident from the preceding considerations that the principle of stimulus generalization will apply in analogous verbal responses as well as in the ones just considered. There will necessarily be female siblings called "sister." There will also be female cousins who not only are surrounded with the same inanimate environment as the actual sister but who, under bilocal residence, live with and behave toward the subject much as sisters do. As a matter of fact, the tendency to call both the mother's brother's daughters and the mother's sister's daughters "sister" yields a coefficient of association of +.73. In an analogous situation the tend-

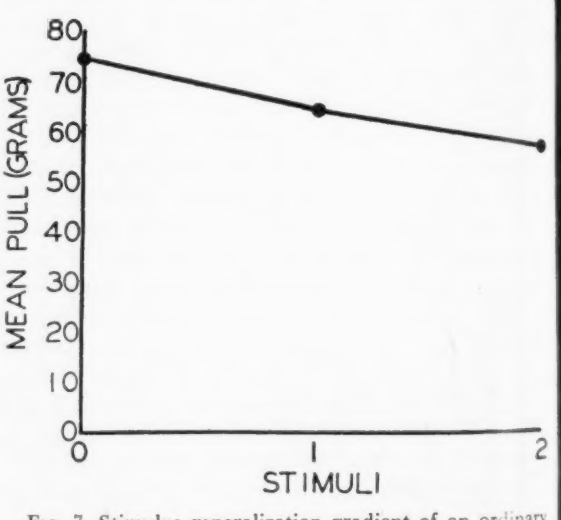

FIG. 7. Stimulus generalization gradient of an ordinary habit (with food reward in rats) to pull in an alley toward lights differing on a logarithmic scale from the light originally associated with food attainment. (Reproduced from Brown.[3])

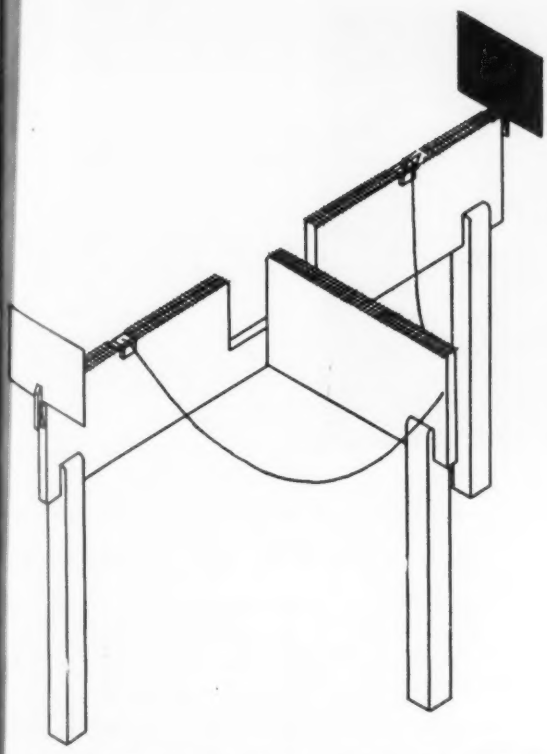

FIG. 8. Representation of the T-maze used by Miller and Dollard in demonstrating that one animal will respond to stimuli produced by movements made by another animal. (Reproduced from Miller and Dollard.[8])

ency to call both the father's sister's daughters and the father's brother's daughters "sister" yields a Q of +.75. Both these values would occur by chance only about once in a thousand times. By a similar analysis, we should expect a stimulus generalization of the response of calling one's own child "daughter" to extend to the daughter of one's sister and of one's brother in cultures having bilocal residence. As a matter of fact, the tables secured from Murdock's 250 cultures at present represented in his Survey yield a Q of +.73, again with a mere chance occurrence of only about one in a thousand.[9]

Glancing back over our examples of the operation of the law of stimulus generalization, we find this principle manifesting itself in the conditioned-reflex salivary secretion of dogs to touches on the skin, in the conditioned-reflex galvanic reaction of men to touches on the skin, in ordinary learned behavior of rats to different light intensities, in the locomotor behavior of rats to the social stimulus of such behavior in other rats, in the interactive fighting behavior of rats, and, finally, in the familial speech reactions of primitive people to relatives, depending on varying residence practices. It is believed that these varied phenomena are demonstrated to be in the same science, just as the various phenomena manifesting the law of gravity are demonstrated to be in physical science. Upon what, except the laws of a given science, depends its essential unity? Whether this science (or these sciences as a group) shall be called "social" or behavioral," or whether a quite new name shall be coined, as Murdock has suggested,[10] is quite irrelevant to science as such. The urgent question is that the science be developed as rapidly as possible.

Someone has said that social or behavioral sciences are now at about the same stage of development that physics was when Galileo began his quantitative investigation of the elementary laws of motion. There is evidently some truth in this statement. On this analogy it would seem that if a

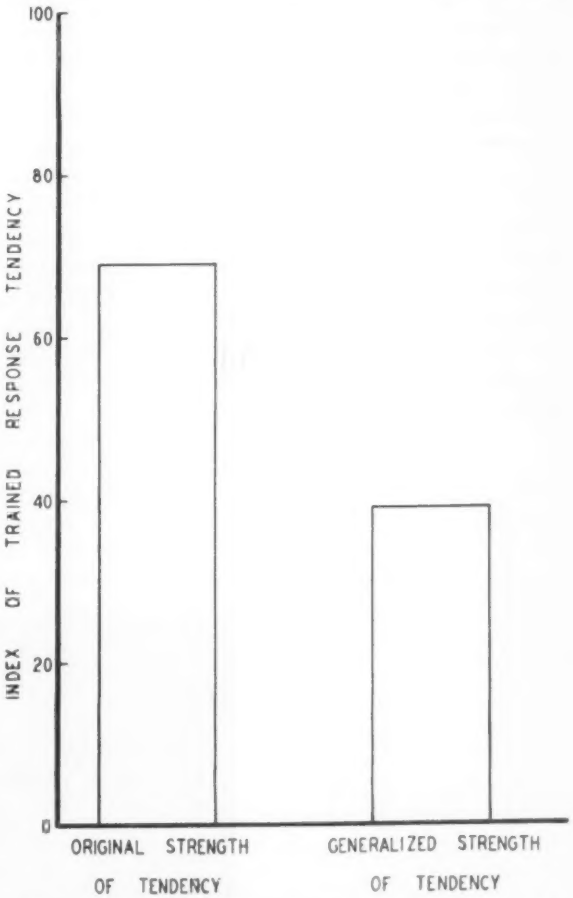

FIG. 9. Diagrammatic representation of the stimulus generalization gradient of the tendency of albino rats trained to take one of two paths on a T-maze (Fig. 8) when tested with a white stimulus animal as originally trained (left-hand column) and when tested with a black stimulus animal (right-hand column). (Derived from Miller and Dollard's Figs. 4 and 5.[8])

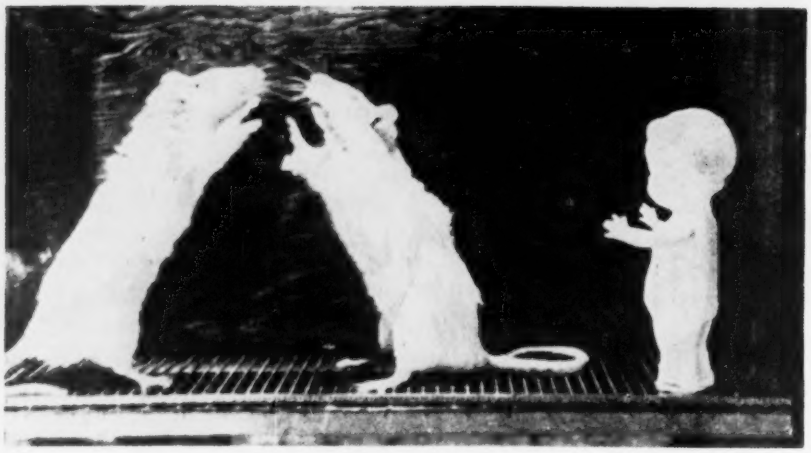

FIG. 10. Rats "fighting" each other on a charged grid as originally trained with the reward of a cessation of the shock, both ignoring the celluloid doll. (Reproduced from Miller.[7])

relatively small amount of effort should be concentrated on the task of searching for and verifying the elementary behavioral laws involved, the social sciences would soon be on their way to the type of development which began in physics with Galileo's productive life. This view is strongly supported by our experience at the Institute of Human Relations at Yale, where much of the work yielding the phenomena described above was performed. However, we must avoid the besetting weakness of Americans to be largely diverted from the pure-science search for primary laws to searches for short-sighted, immediate rule-of-thumb solutions to practical problems.

References

1. ANREP, G. V. *Proc. Roy. Soc.*, London, B, 1923, 94, 404.
2. BASS, M. J., and HULL, C. L. *J. Comp. Psychol.*, 1934, **17**, 47.
3. BROWN, J. S. *J. Comp. Psychol.*, 1942, **33**, 209.
4. HOVLAND, C. I. *J. Gen. Psychol.*, 1937, **17**, 125.
5. HULL, C. L. *Principles of Behavior*. New York: D. Appleton-Century, 1943.
6. ———. *Psychol. Rev.*, 1950, **57**, 173.
7. MILLER, N. E. *J. Abnorm. Soc. Psychol.*, 1948, 43, 155.
8. MILLER, N. E., and DOLLARD, J. *Social Learning and Imitation*. New Haven: Yale Univ. Press, 1941.
9. MURDOCK, G. P. *Social Structure*. New York: Macmillan, 1949.
10. ———. *Sci. Mon.*, 1949, **69**, 377.
11. PAVLOV, I. P. *Conditioned Reflexes* (trans. by G. V. Anrep). London: Oxford Univ. Press, 1927.
12. YERKES, R. M., and MORGULIS, S. *Psychol. Bull.*, 1909, **6**, 257.

FIG. 11. A rat on the charged grid in the absence of the normal object of attack generalizes the learned response to the celluloid doll, thus demonstrating the falling gradient of stimulus generalization (*cf.* Fig. 10). (Reproduced from Miller.[7])

Significance of the Newfoundland Surveys in Human Nutrition

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ADVANCES made in the science of nutrition over the past few decades have brought us much closer to the optimal goal of the nutritionist which, as has been stated elsewhere,[10] may be defined as "the development of a diet which supplies all nutrients both in respect to kind and amount and in proper state of combination for all physiologic processes from conception to the death of the organism." It will be noted that the scope of this definition covers the entire life span. Students of nutrition can cite numerous experiments showing that faulty diet can result in (*a*) malformation of the developing embryo, (*b*) poor growth during the early stages of life, (*c*) imperfect maintenance of the adult organism, or (*d*) in failure to meet properly the special needs of reproduction and lactation. In more recent years, through the studies of Professor H. C. Sherman at Columbia University,[9, 24–26] it has been possible to show that a diet previously regarded as satisfactory for the "normal" life span of rats and containing what had been regarded as proper amounts of calcium and riboflavin and vitamin A can still be improved through greater supply of these essentials so as to contribute to greater longevity and continued maintenance of health and vigor.

This vast body of knowledge, which came chiefly from animal experimentation, is before us for consideration. One may properly ask how close we can now come to this ideal of optimal nutrition for the human species. It must be evident that it is much more difficult to obtain precise data of this sort pertaining to man. In this difficulty resides much of the controversy and many of the differences in viewpoint evident in the discussions of this theme by various workers in the field. Some clinicians take the view that unless an individual shows definite, obvious signs of disease—dry eye, suggestive of lack of vitamin A; hemorrhages, suggestive of lack of vitamin C; et cetera—there is no clinical entity to discuss, and from this point of view the adequate diet is one that just prevents the appearance of any such symptoms. Another group of clinicians, represented by those who have seen many cases of deficiency diseases and who have been impressed by the relatively large amounts of the dietary factors needed to produce good therapeutic results, takes quite a different view. In any discussion of amounts of various dietary factors—vitamins, for example—which individuals should have for good health, this particular group tends to emphasize quantities that are quite high compared with those suggested by other nonclinical workers in the field. The reason for this is rather obvious, namely, the clinical experience of these physicians in treating deficiency disease. Another viewpoint is that taken by nonclinical nutritionists interested in problems of public health, particularly what to recommend as the best possible diet for supporting optimal growth of children and maintenance of good health of the adult, what amounts of various dietary essentials are needed to meet the normal physiologic requirements of reproduction and lactation. The amounts of the dietary factors suggested by this last group of workers as appropriate standards to use in evaluating human dietaries and as the goals of food supply

for optimal human nutrition tend to be between those suggested by the two clinical groups, with perhaps a few exceptions.

We have a large body of knowledge concerning the conditions that must be met to secure optimal nutrition in animals but are relatively uninformed regarding such needs for man. There are many reasons for this. It is much more difficult to set up and carry through an ideal nutritional experiment on human subjects. When experimenting with animals the individuals are fed very carefully devised experimental diets which are as complete as possible except for a single factor. This enables the investigator to draw conclusions regarding the role of the dietary factor in question in preventing the appearance of symptoms of deficiency that may be observed. Consider on the other hand the situation facing the investigator who must work with human subjects. When a new dietary factor has been discovered through animal experimentation, it is desirable to determine by some means whether it is truly a dietary essential for man. Observations bearing on this point have to be made on clinical cases coming to the attention of physicians. If the symptoms exhibited by these human subjects are at all similar to those seen in the deficient animal, it is reasonable for the physician to try administering to these patients the dietary factor in question to see whether the symptoms are alleviated. If they are, then one is in a better position to say whether that factor is truly important for man. Numerous instances in the clinical literature concerning deficiency diseases might be cited to illustrate this point. One example is pellagra, which is particularly prevalent in populations using corn as a staple food, and is characterized by a complex of symptoms, some referable to the alimentary tract, others to the skin, and still others involving the nervous system. In this complex of systems exhibited by patients with pellagra there is often seen a cracking of the lips at the angles of the mouth, a condition called cheilosis. As long as pellagra was treated by natural foods, which would of course furnish many specific dietary factors, this symptom was alleviated along with many of the others cited above. When nicotinic acid was demonstrated to be the antipellagra factor in the sense that Dr. Joseph Goldberger had in mind many years ago, critical trials of this substance on pellagra cases showed that, whereas most of the symptoms already mentioned were relieved, this cheilosis was not affected. When, however, the patients were then given in addition to the nicotinic acid a sufficient amount of the pure B vitamin riboflavin, the cheilosis was cured.[23] Obviously, then, the cases as originally seen by the clinician represented not merely a marked deficiency of nicotinic acid, but a lack of riboflavin and perhaps other vitamins as well. It must be evident that as long as advances in this field have to depend essentially upon the treatment of obvious deficiency disease, the advance will be slow. Such patients cannot be regarded as exactly comparable to groups of experimental animals fed purely artificial rations of known composition and therefore very carefully controlled. On the other hand, it must be recognized that any observations that are made on a population group of individuals exhibiting definite signs of deficiency disease can constitute valuable contributions to our knowledge of the subject as a whole. Observations made on large population groups can throw considerable light on the extent to which conditions of malnutrition exist in the population and therefore the extent to which nutritional problems are presented by the population groups in question.

One may well inquire whether it is possible to determine the existence of these deficiency states at a much earlier stage by the perfection of more delicate diagnostic methods. If this could be done, then it should be possible to detect instances where the deficiency disease is developing but has not reached the point where obvious and gross symptoms are manifest to any observer. This area of research needs extensive cultivation. Considerable advances have been made along this line, but much still remains to be done.

The importance of this problem is well illustrated by guinea pig experiments performed by one of us and an associate.[7] In the absence of a dietary source of ascorbic acid the guinea pig, like man, develops scurvy. Measurements of the amount of ascorbic acid needed by the guinea pig to prevent gross outward obvious signs of scurvy have been made. Such symptoms are painful joints, bleeding of the gums, retarded growth, and assumption of a peculiar body position, or stance. In addition to these gross symptoms, careful examination of body tissues of the guinea pig subsisting on a diet low in vitamin C has shown that characteristic changes occur in the connective tissues, and particularly that the odontoblastic cell layers in the roots of the teeth suffer considerable derangement. If, therefore, groups of the animals are fed diets with graded amounts of vitamin C, allowed to eat the experimental ration for fourteen days, and then are sacrificed and the roots in the incisor teeth prepared for histological examination, it is possible to demonstrate various stages in the derangement of the cells of the odontoblastic layers, ranging all the way from perfect organization down to complete chaos and disinte-

gration of the entire layer. If the amount of ascorbic acid required by the guinea pig to keep the odontoblastic cell layer in perfect condition is measured, it is found to be approximately twice that needed to prevent the aforementioned appearance of gross signs of scurvy. Many of these stages of derangement in the odontoblastic cells are seen in animals which do not show any of the outward signs of scurvy. One may inquire how properly to designate the condition present in such a guinea pig. Perhaps it could be called "latent scurvy;" or it could be labeled "vitamin C subnutrition;" or it might be called "vitamin C undernutrition;" or some other name could be devised for it. It is evident that the investigators who have used such terms in the scientific literature have been attempting essentially to find a name for a lesser degree, or early stage of dietary deficiency as yet not outwardly determinable by any known tests. It seems reasonable to contend that *optimal* nutrition for the guinea pig, so far as ascorbic acid is concerned, should mean maintenance in healthy state of these odontoblastic cells as well as of all other cells, and in addition the vital chemical processes that go on normally within the organism. Just as the pathologist often reveals the true nature and extent of diseases of a nonnutritional nature which the clinician has been unable to diagnose completely due to lack of symptomatology, so may one have a lack of symptomatology in the subclinical, marginal nutritional deficiency syndromes, while at the same time pathological changes in tissues may have developed or be developing. In this connection the pathologist has a field to develop that requires the recognition of functional pathology resulting from lack of optimal cell nutrition.

In view of the foregoing, if any human population group shows obvious signs of deficiency disease in a certain fraction of its individuals, it is fair to conclude that the dietaries are seriously faulty in certain respects, and that probably many other individuals in that group suffer from some malnutrition of the hidden type illustrated by the guinea pig example cited above. Kruse has discussed this problem in some detail, [12–14] and Dann and Darby[8] have made valuable suggestions concerning it in their review article on this subject.

Inasmuch as investigators in this field are unable, except under very unusual circumstances, to carry out on human subjects carefully controlled studies of the type that can be conducted on experimental animals, it is necessary to adopt survey techniques, to collect data on large numbers of individuals, and then to study them and evaluate the findings by statistical methods. A valuable attack on this problem would be made if one could deal with a reasonably large population group located in a fairly definite and characteristic area, where the population could be surveyed by a group of experts acquainted with the latest techniques available for such work, where certain practical public health measures might then be given a trial, and where, finally, another survey of the same group could be made after a reasonable period. The results of any such investigation as this should be of great significance in human nutrition. Two such initial surveys were indeed made in Newfoundland in 1944, and both of them were repeated later, in 1948. The more extensive of the two, made on the east and south coasts, was intended to reveal what might be the "general nutrition" of the people and involved examinations of a total of 868 persons.[1] The other one dealt with 39 families in the fishing and logging community Norris Point, located on the west coast, and was intended to cover a population sample characterized by relatively low socioeconomic status and likely to be in the poorest state of nutrition.[18] By order of the Newfoundland government and on recommendation of the Council on Nutrition of the Newfoundland Medical Association, all flour entering the country after July 1, 1944, had to be enriched according to U. S. Federal standards. The smaller survey was made in June 1944, the larger one in August of that same summer. The first shipment of enriched flour actually arrived while the larger survey was being made. The resurveys four years later afforded a study of the remedial effects, if any, of the measures taken by the Newfoundland authorities.

Since 1912, beginning with the work of Little,[15] who made observations at the Grenfell Hospital at St. Anthony's in northern Newfoundland, cases of deficiency disease have been reported in this area. Little made the comment in his paper[16] that Dr. Grenfell repeatedly had encountered beriberi in his work on the Labrador coasts. Little himself reported the diagnosis of beriberi in 220 out of 5,000 cases seen in the outpatient department at the hospital at St. Anthony's. From time to time various other workers made visits to Newfoundland and reported observations of one kind or another bearing on this theme. In 1921 Appleton[2] described deficiency diseases in Labrador and northern Newfoundland. In 1930 Aykroyd,[4] then house surgeon in the general hospital at St. John's, reported not only on beriberi but on scurvy and other deficiency states.

Scurvy in severe form was infrequently encountered; night blindness was common and other signs of deficiency of vitamin A were frequently encountered. Many of the

fishermen in Newfoundland suffer with "salt water boils" on the wrist and forearms. This painful complication supposedly related to faulty diet is mentioned by Kipling in his story of the Newfoundland banks *Captains Courageous*. The hero developed a crop of "gurry sores" which made of him a "full blooded banker."

Aykroyd,[3] Mitchell,[19] and Vaughn and Mitchell[28] made observations on the dietaries in use in northern Newfoundland. It was quite evident from these studies that the dietaries in common use in this area were faulty from several points of view. Steven and Wald[27] also demonstrated in a field study that considerable vitamin A deficiency exists in this population.

Newfoundland lies in the Gulf of St. Lawrence and has an area of 42,000 square miles, which is a little less than that of the state of New York or of England. The population in 1944 was estimated to be about 320,000 people. The land is characterized by poor fertility, and the poverty of many of the people is very great. A large proportion of the able-bodied population is engaged entirely in fishing. Many of the people live in remote areas that are without adequate means of transportation. For all practical purposes Newfoundland is essentially "a one-crop area with all of the regrettable consequences that this imposes."

As stated in the first report[1] of the more extensive survey: "The government of Newfoundland has been acutely conscious of the high incidence of disease and the faults in the diet of the country." In 1944, at the invitation of Sir John Puddester, Commissioner for Public Health and Welfare, in response to a recommendation made to the government of Newfoundland by the Nutrition Council of the Newfoundland Medical Association, survey teams were invited to make studies of this situation. The larger survey was carried out August 13–30, 1944, by a team representing the Royal Canadian Army Medical Corps, the Pediatrics Department of the University of Toronto, the Medical Services Indian Affairs Branch of the Canadian government in Ottawa, the Medical Research Council of Great Britain, New York University College of Medicine, Milbank Memorial Fund of New York City, Public Health Research Institute of the City of New York, the U. S. Public Health Service (Washington), the Mayo Foundation (Rochester, Minnesota), and the Harvard Medical School.

> The present survey [conducted by this particular team] was designed to examine a sample of the population of Newfoundland for clinical and biochemical evidence of abnormality relating to nutrition. The subjects of the study were 868 unselected people in St. John's and several outports who voluntarily submitted to examination after word of mouth publicity had been given as to time, place and purpose of the examination. . . . The satisfactory cooperation obtained from all of these people contributed importantly to the success of the project. The individuals examined came from 6 different communities. . . . It is to be noted that this survey was undertaken in a season of the year (August) when the diet is relatively superior. All previous writers on nutritional disease in Newfoundland have commented on the accumulation of cases of such disease in later winter and spring. Fresh fish and garden produce become available in the summer months; likewise wild berries supplement the diet, although only to a small extent.

Space does not permit discussing this report in great detail. Let it suffice to point out that

> Evidences of nutritional deficiency due to the lack of vitamin A, riboflavin and ascorbic acid, were seen with great frequency. Evidence due to lack of thiamine was encountered much less frequently but still in an appreciable number of the people. Mild, acute and chronic niacin deficiency was of frequent occurrence in the population examined. Signs of rickets were seen in comparatively few subjects. Only moderately low hemoglobin values were found in a proportion of the population and no evidence of protein deficiency was encountered.

This particular report is noteworthy in that it contains numerous colored photographs illustrating various signs of deficiency seen in many persons examined. These pictures show the outward effects of these dietary deficiencies exhibited by some individuals in this population group.

The available food supply was studied with respect to its adequacy, and the results were compared with the daily dietary allowances recommended by the National Research Council.[21] On this basis the food supply was judged to be adequate in calories, protein, and fat, but to be extremely deficient in vitamin A, riboflavin, and ascorbic acid. The supply of thiamine, although relatively better than that of the above-noted nutrients, was still inadequate. The amount of niacin was at the recommended level. The supply of iron was somewhat below that recommended, and the calcium supply was extremely low. The report emphasizes that these comments refer to average figures, that few people really eat such average amounts, and that "the food actually consumed by many persons is undoubtedly less satisfactory than was suggested by the average figures."

Some chemical analyses of the blood were made as part of both the larger and the more limited surveys. In the case of the former the new microchemical methods devised by Drs. O. H. Lowry and O. A. Bessey[17] were used. These workers developed chemical techniques applicable to extremely small amounts of blood such as fieldworkers might readily obtain from a prick of a finger or of the ear lobe. The drops of blood are drawn into fine capillary glass tubes, which are then sealed and sent to a laboratory, where the microchemical analyses are

performed. This technique proved its value in this study and is now a useful addition to the armamentarium of biochemists who wish to participate in field nutritional surveys. The official report of the larger survey states that "on the whole a close correspondence is found between the food deficiencies and the chemical and the clinical signs of malnutrition."

The first of the surveys that covered 40 families in Norris Point[18] involved examinations of 39 women, 34 children from three to six years of age, 24 ranging from ten to fourteen years of age, and 6 men. Of the women examined, 6 were pregnant and 14 lactating. Ten additional men seen in clinic were included in the survey.

> On the basis of medical histories, physical examination, hemoglobin and plasma protein determinations, the incidence of nutritional deficiency among the women was as follows: apparent, 61 per cent; suggestive 31 per cent; questionable or none, 8 per cent. Children were similarly evaluated and showed the following: preschool children—apparent deficiency, 23 per cent; suggestive, 47 per cent; and questionable or none, 30 per cent; preadolescent children—apparent deficiency, 29 per cent; suggestive, 29 per cent; and questionable or none, 42 per cent. Of the men, 44 per cent had apparent, 44 suggestive, and 12 per cent questionable or no deficiency. Multiple deficiencies were the rule, and those most frequently encountered were vitamin A, riboflavin, and iron in the women and children; vitamin A and riboflavin in the men.

The last sentences of this report are particularly interesting: "The findings indicate a need for remedial measures. An isolated community such as Norris Point is an ideal laboratory for investigation of the value of various therapeutic procedures."

When the flour enrichment program was begun in the United States, many physicians and others considered it to be sufficiently supported by scientific considerations. Dr. Wilder[29] has commented that

> Some scientists objected on the ground that the program was not needed in our well-fed country; others on the ground that more was needed than the program could provide. Doubts were thus created, and as time passed interest waned until recently the attitude of many who were formerly warm supporters of the program has been one of rather passive acquiescence, with little active faith in the public health advantages of enrichment.

As has been mentioned, any study of this sort will be much more significant if a resurvey can be conducted on the same group and in the same area after a reasonable period of time has elapsed to permit remedial measures to exert their effects, if any. Such resurveys were actually conducted in Newfoundland in 1948 by practically the same teams that made the first surveys.[5, 11]

The following statement in the report of the original project in large measure applies very well both to this survey and to the smaller one made at Norris Point.

> The resurvey here reported was undertaken in the hope of being able to evaluate the effect on health of several measures undertaken by the government of Newfoundland to improve nutritional conditions. . . . The season of the year was the same. . . . The same localities were studied as before, with a few exceptions to be noted later. The number of persons examined was the same (868 and 868; 113 in 1944 *vs.* 100 in 1948, and 26 additional persons) and the same criteria were adopted for judging abnormalities. A feature of this study which probably is unique in nutritional investigation, is that 227 individuals examined in 1944 could be re-examined by the same examiners four years later.

Data on food consumption were obtained covering the intervening years. It appears that the supplies of evaporated milk increased by about 80 per cent between 1944 and 1948. Imports of fruit, fresh and canned, mostly citrus fruit and juices, almost doubled between the fiscal years 1943–44 and 1944–45, but fell somewhat in the later years. Careful examination of these data led to the conclusion that

> Assuming consumption only by civilians the amount per head per day for the fiscal year 1947–48 was 0.7 ounces (20 grams), which would represent a contribution to the daily provision of ascorbic acid of no more than 7 milligrams. Other data suggest that eggs, fats and oils, and green leafy vegetables may have been imported in increasing quantities in the later years, but definite statements regarding import trends in the case of these and some other foods and food groups cannot be made because of annual fluctuations.

Following the first survey, numerous measures were taken aimed at improving the nutrition of the people. They comprised a

> program for public education in nutrition, distribution in the schools of a hot milk drink, distribution of cod liver oil, distribution to certain individuals of concentrated orange juice and improvement of the quality of margarine and white flour through fortification of the margarine with vitamin A and enrichment of the flour with thiamine, riboflavin, niacin, iron and calcium.

The public education program included many activities of interest. There was increased emphasis on nutritional consideration in the radio advertising of processed foods, the importance of nutrition in the prevention and treatment of tuberculosis was emphasized, and an excellent colored motion-picture film was prepared in 1948 by the Newfoundland Tuberculosis Fund Association. Other efforts to teach improved dietary habits had been made by the Department of Public Health and Welfare of the Department of Education and various private organizations such as the Red Cross. In 1947 an experienced nutritionist was added to the staff of the Department of Public Health and Welfare. She worked through the teachers in the schools,

women's civic bodies, and the public health nurses. A pamphlet on nutrition designed especially to meet conditions in Newfoundland was distributed in 1947 to all schoolteachers. Still other methods of approach included weekly radio talks, press articles, and talks to adult education groups. It is important to mention that little of this educational activity really got under way before the early months of 1947. "That it was beginning to have effects in 1948 is indisputable, but that it had significantly influenced the nutritional condition of the people by the time of our resurvey is doubtful. Dietary habits change slowly."

Another line of activity consisted in the government's undertaking a full-scale school-feeding program. This involved the free distribution to a large majority of the schools of dried nonfat milk powder, which was flavored with cocoa and sweetened with sugar; from it a hot drink was prepared which children liked and "consumed in cups or mugs brought to school." It is estimated that the average serving was 4–6 ounces (120–180 ml). This hot-drink program was initiated in a small number of schools in 1946. The formula was improved and distribution was extended in the school year beginning September 1947.

Other materials were also distributed free of charge during the school year 1947–48, particularly cod liver oil and concentrated orange juice. This latter item went to expectant and nursing mothers and infants under one year of age. It was said to contain 220 mg of ascorbic acid per 100 g, and the total quantity dispensed was 880 gallons. It is estimated that each individual received something like about 1.1 mg of ascorbic acid daily for one year if the distribution is figured as spread over the whole year period. Beginning in January 1945, imported margarine was fortified with vitamin A. At this time the required content of vitamin A was set at 30 international units per gram; 45 international units per gram was the required amount after January 1946. It was estimated that the contribution of the vitamin through margarine "would double the estimated provision for added vitamin A in 1944." In any case, "the increase represented a very significant alteration of the nutritional situation." Following the recommendation that the white flour should be enriched, the government of Newfoundland established appropriate regulations to this end beginning with the fiscal year 1944–45, and all wheat flour imported after that date was of the enriched variety.

An additional requirement was imposed in October 1947, namely the inclusion of bone meal in an amount sufficient to provide not less than 500 mg of calcium per pound. No flour was milled in Newfoundland; hence, except for an insignificant amount introduced by smugglers and a very small amount of packaged flour used for cake and pastry-making, all white flour consumed had been enriched. Except for the city of St. John's, which received enriched flour as early as April, 1944, this happened after our survey of 1944. In point of fact, we witnessed in August, 1944, the arrival at the outports of Fortune Bay of the first shipments of enriched white flour.

What was the effect of these changes as shown in the resurvey made in 1948? Space does not permit a detailed discussion of the findings. Let it suffice to quote from the summary of the resurvey report of the larger project[5] as follows:

> It can be said that those signs and symptoms of malnutrition which could have been expected to decrease in prevalence as a result of the fortification of margarine with vitamin A and enrichment of flour with riboflavin, niacin, and thiamine were less frequently encountered and less severe in 1948 than in 1944, whereas the prevalence of lesions which could not have been effected by these measures remained unaltered or actually increased. This does not mean that other programs in part recommended by the group in 1944 are of less importance than the two which apparently by 1948 had produced results. [It should be added here that in Norris Point, however] few changes occurred relative to vitamin A.

Elsewhere Dr. Wilder[29] has commented on this matter as follows:

> The quality of life had improved in Newfoundland. So, too, to judge by such criteria as mortality rates, had the quantity of life. The reported death rate from all causes fell from 12.1 to 10.5 per thousand of the population. The number of deaths from tuberculosis decreased sharply and the rate of stillbirths and the death of infants less than 1 year of age was strikingly reduced. . . . Other things occurred which may have influenced the nutrition of the population and with it the public health. The people have much more money to spend and could have purchased more or better food. However, the data indicated that this was not the case. They had more money but they did not purchase better food. Not all the outward signs of malnutrition had decreased in prevalence. Only those had lessened which could have been effected by addition of vitamin A to the margarine, and of riboflavin, niacin, and thiamine to the flour. There was no improvement and indeed a worsening with respect to the problems of lesions relatable to deficiency of vitamin C (ascorbic acid). [It was quite evident that] the people simply hadn't bought more fruits and vegetables. Had they done so the values of vitamin C in their blood would have shown it. . . . More milk was procured in cans but less was produced in many outports with a net effect of questionable importance. . . . The program of public education in nutrition in the short time available and the distribution in the schools and to pregnant women and infants of skimmed milk, cod liver oil and concentrated orange juice in the small amounts distributed could have had but little effect, or otherwise the urines would have shown more vitamin C, and the prevalence of outward signs of vitamin C deficiency would have lessened. We thus inevitably must give the principal credit for the improvements observed in Newfoundland between 1944 and

1948 to enrichment of the flour and fortification of the margarine.

What are some of the lessons to be gleaned from this for people living in the United States, who have frequently been pointed to as being about the best-fed people in the world and who have "vitamins to spare"? In 1939 one of us[6] wrote a paper emphasizing the importance of getting more vitamin B-1 into the American diet. This was prior to the enrichment program which took form in 1941. The recent study of Phipard and Stiebeling[20] reviewed the foods consumed per head in the United States during different periods. In the period 1935 through 1939, before enrichment, the diet provided little in the nature of a margin of safety with respect to the vitamins thiamine, riboflavin, and niacin, and the mineral nutrients iron and calcium. There seemed to be liberal amounts of vitamin C, vitamin A, protein, and calories. According to Phipard and Stiebeling, however, since shortly after 1941 the food consumed per head has more nearly provided the necessary factor of safety with respect to thiamine, riboflavin, niacin, and iron.

There can be no question that malnutrition in most areas of the United States in the years 1935–39 undoubtedly was less than that in Newfoundland. This, however, should not give us a feeling of complacency. When considering statistical data one must always remember that an average necessarily means that some figures are above and some below the average. Every survey that has been made on groups in the United States has shown some individuals to have had some form of malnutrition. Some of these people may have been affected more severely than others, even though the general incidence as indicated by the average may have been relatively low. In a population such as that characteristic of hospitals in New York City, notably that seen by Dr. Jolliffe in wards of Bellevue Hospital, it was not unusual formerly to encounter many cases of beriberi and pellagra. Dr. Jolliffe has noted, however, that since 1941, when a very large proportion of the bread became enriched, and by 1943 the severe nutritional diseases which result from lack of thiamine and niacin had disappeared completely. It is interesting to note also that this improvement in New York City, just as in Newfoundland, was unaccompanied by any lessening in the prevalence of abnormalities related to lack of vitamin C. Jolliffe attributed this not to a general improvement in the diet made possible by increased money income, but specifically to the change made in the bread.

Another example that might be cited relates to pellagra in the Southern states. Formerly this disease was very prevalent in that area; in recent years it has almost disappeared. It can of course be argued that this improvement has been related to war prosperity and the purchase of better foods by the inhabitants. But Dr. Wilder points out[29] that "the Newfoundland experience prompts me to question that interpretation and to give the credit for the change to enrichment of the flour and corn meal." As Dr. Wilder says further:

> What is good for Newfoundland is also good for us. White flour and refined corn meal furnish together a relatively enormous proportion of the calories of the diets of the less privileged sections of the population, and the enrichment of these calories with niacin, riboflavin, and thiamine as was shown in Newfoundland provides a way of immediately correcting the principal shortcomings of such diets. On the other hand, as was true of Newfoundland, the effect of a brief increase in money income, even when accompanied with nutrition education, may not effect improvement of the diet.

This should point up to all workers in public health the importance of continuing work in nutritional education while recognizing that the results of such education may not be seen very soon.

Solution of the malnutrition problem, if we may call it that, requires, of course, advances on many fronts. Researches into the basic principles and facts of the science of nutrition must go on. While this is being done the existing knowledge must be applied wherever possible. This means that educational efforts must continue and be prosecuted with even greater intensity. It has been estimated that something like thirty years of educational effort in the Philippine Islands were required to bring about any substantial change in the food habits of the Philippine population sufficient to reduce significantly the incidence of beriberi. More recently, methods have been found for enriching rice, and field trials are now being conducted in certain areas of the Philippines to determine the value of the distribution and consumption of enriched rice in bringing about better over-all nutrition. At least one report[22] of this study has already appeared, and others will doubtless be forthcoming in the future. If rather quick results are indeed obtained by the substitution of enriched rice for the older polished variety, the importance of this enrichment program will be quite obvious; the time required for its desirable public health results to appear may be contrasted with the estimate of thirty years needed when reliance is placed primarily on educational work. We shall always have the educational problem with us, but the results of educational efforts will be more difficult to evaluate.

It would of course be unfair to claim that all the improvements noted in Newfoundland, the Philip-

pines, or the United States are due entirely to the enrichment of flour and the use of vitaminized oleomargarine. Other factors necessarily play some role. The low economic level of a population must play a significant part. Then there is the question of the value of the food supply as a whole, in contrast to that of just a few items. Food habits play an important part, and it is generally recognized that food habits are difficult to change; it is probably easier to get results by starting the educational process with the young child and continuing it into the individual's adult life. Final solution of the problem requires the continued cooperation of many groups of workers, nutritionists who study the science, physicians and public health workers who apply it in the prevention and treatment of disease, home economists who study practical food problems of the home and elsewhere, educational workers who help develop proper food habits, economists, psychologists, and others. Food technologists also are important, for they are the ones to tell us whether methods of producing, processing, transporting, and storing foods result in losses of nutritive values, and what might be done to reduce such losses to a minimum. Addition of specific nutritive factors to certain foods, illustrated by vitaminizing margarine and enriching white flour, constitutes a relatively new way of improving the nutritive value of foods, one that is made possible by the perfection of methods for chemical synthesis of many of the vitamins. In this connection it seems pertinent to repeat here what one of us (GRC) wrote in 1939[9] on this subject:

> The American people should, if possible, obtain their needed vitamins from foods rather than from pharmaceutical preparations. The preparations available in the local drug store should be those desired by the physician in the treatment of special cases which involve vitamin deficiencies. It certainly is not the ideal solution of our basic problem of improving the general nutrition of the American people to advocate that they make greater use of the various vitamin B preparations now available essentially as pharmaceuticals because the people most in need of the vitamins happen to be those with the least money to spend for necessities. We are thus brought around to the need for improving our important widely available and widely accepted foods. If certain basic energy-rich staples are made the appropriate carriers of the needed vitamins, as was pointed out previously, this single step affects more persons in the desired way than any other that can be devised.

The results of the nutritional surveys and resurveys made in Newfoundland and discussed briefly here certainly support this point of view. In our opinion they constitute a demonstration of the value for public health of at least two particular food fortifications now widely accepted in the United States—the addition of vitamin A to margarine and the enrichment of white flour.

References

1. Adamson, J. E., *et al. Canadian Med. Assoc. J.*, 1945, **52**, 227.
2. Appleton, V. B. *Am. J. Pub. Health*, 1921, **11**, 617.
3. Aykroyd, W. R. *Irish J. Med. Sci., Series 6*, 1928, **28**, 161.
4. ———. *J. Hyg.*, 1930, **30**, 357.
5. Aykroyd, W. R., *et al. Canadian Med. Assoc. J.*, 1949, **60**, 329.
6. Cowgill, G. R. *J. Am. Med. Assoc.*, 1939, **113**, 2146; see particularly p. 2151.
7. Dann, M., and Cowgill, G. R. *J. Nutrition*, 1935, **9**, 507.
8. Dann, W. J., and Darby, W. J. *Physiol. Rev.*, 1945, **25**, 326.
9. Ellis, L. N., Zmachinsky, A., and Sherman, H. C. *J. Nutrition*, 1943, **25**, 153.
10. Elvehjem, C. A., and Krehl, W. A. *J. Am. Med. Assoc.*, 1947, **135**, 279.
11. Goldsmith, G. A., *et al. J. Nutrition*, 1950, **40**, 41.
12. Kruse, H. D. *Milbank Memorial Fund Quar.*, 1942, **20**, 245.
13. ———. *Ibid.*, 1948, **26**, 41.
14. ———. *Ibid.*, 1949, **27**, 5.
15. Little, J. M. *J. Am. Med. Assoc.*, 1912, **58**, 2029.
16. ———. *J. Am. Med. Assoc.*, 1914, **63**, 1287.
17. Lowry, O. H., and Bessey, O. A. *Fed. Proc.*, 1945, **4**, 268.
18. Metcoff, J., *et al. J. Lab. Clin. Med.*, 1945, 30, 475.
19. Mitchell, H. S. *J. Am. Diet. Assoc.*, 1930–31, 6, 29.
20. Phipard, E., and Stiebeling, H. K. *J. Am. Med. Assoc.*, 1949, **139**, 579.
21. *Recommended Dietary Allowances. National Research Council Reprint and Circular Series No. 115*, January 1943.
22. Salcedo, J., *et al. J. Nutrition*, 1949, **38**, 443.
23. Sebrell, W. H., and Butler, R. E. *U. S. Pub. Hlth. Rpts.*, 1939, **54**, 2121.
24. Sherman, H. C., and Campbell, H. L. *Proc. Nat. Acad. Sci.*, 1945, **31**, 164.
25. Sherman, H. C., Campbell, H. L., and Lanford, C. S. *Proc. Nat. Acad. Sci.*, 1939, **25**, 16.
26. Sherman, H. C., *et al. Proc. Nat. Acad. Sci.*, 1945, **31**, 107.
27. Steven, D., and Wald, G. *J. Nutrition*, 1941, 21, 461.
28. Vaughn, M., and Mitchell, H. S. *J. Am. Diet. Assoc.*, 1933, **8**, 526.
29. Wilder, R. M. Before Conference on Nutrition in Newfoundland sponsored by the Nutrition Foundation, New York City, Apr. 4, 1949.

Control of Sex in the Honeybee

STANLEY E. FLANDERS

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THE method by which sex is controlled in the honeybee has for many years been a problem defying solution. An adequate explanation of this phenomenon in an animal as small as the bee must be based largely, if not entirely, on circumstantial evidence, the critical organs involved being too small and delicate for analytical testing. The fact that the queen bee can function in a normal manner only as an integral part of a highly organized society also militates against the successful use of experimental methods.

The internal structures concerned in the control of sex of the honeybee have been described by Breslau,[1] but all conjectures concerning the *modus operandi* have failed the test of logic and experience. The theory presented herein is based on studies of fresh dissections of queen honeybees* and females of other hymenopterous species, and on years of practical experience in controlling the sex of individuals in mass cultures of nonsocial species of Hymenoptera.

The basis for sex control in the honeybee is well known to beekeepers. The drones, like normal hymenopterous males, are derived from unfertilized haploid eggs. The workers and queens, like other hymenopterous females, are derived from either unfertilized or fertilized eggs, provided such eggs are diploid. The diploid egg in the honeybee is almost always the result of fertilization.

According to Manning (1949), femaleness in the honeybee is a product of a balance between a diploid autosome set of thirty chromosomes plus an X-chromosome, whereas maleness is an effect of a haploid autosome set of fifteen chromosomes plus an X-chromosome. In the formation of sperm the X-chromosome is discarded, so that each sperm has only a set of fifteen autosomes.

*Queen bees obtained through the courtesy of J. E. Eckert, professor of entomology and apiculturist in the California Agricultural Experiment Station.

Under natural conditions, fertilization of the egg in the social organization of the honeybee is determined by environmental factors. The primary stimulus that results in the fertilization of an egg as it passes through the oviduct of the queen originates externally. The active agent, however, in determining the proportion of the population that is to become male or female is the worker bee. The queen is merely a mechanism of reproduction, which is under the control of the workers.

Preference of Comb Cells by Queen Bee Based on Fertilization of the Egg

The honeybee has been under domestication for centuries, but beekeeping has developed into an industry only within the past hundred years, following the development of methods for forcing the bees to construct straight honeycombs. This is accomplished by placing in the hive a prefabricated plan known to beekeepers as a "comb foundation," a sheet of beeswax embossed to form ridges which the worker bees build up to form the walls of hexagonal brood (worker) cells, five to the inch. The use of comb foundations also enables the beekeeper to regulate in part the sex ratio of the hive and thus increase the honey-producing capacity of the colony. When conditions cause brood rearing to be accentuated, however, the workers construct, here and there on the brood comb, small groups of cells of larger size, thus giving the queen two types of cells in which to lay her eggs.

The deposition of an egg in a brood cell by the queen follows her antennal inspection of the inner walls of the cell. This procedure requires only a few seconds, so that it is possible for a queen, in one day, to deposit 2,000 eggs in as many cells. With a mated queen, practically all the eggs develop into females, which become workers if underfed and queens if well fed. However, when a mated queen deposits an egg in a cell of greater diameter than

that to which she is accustomed, that egg as a rule develops into a male bee, or drone.

Observations by Grout[8] on oviposition in a hive containing three sizes of brood cells (normal size, 857 cells per square decimeter, and two larger sizes, 763 and 706 cells per square decimeter) indicated that the queen prefers to deposit fertilized eggs in the normal-sized cells. Fertilized eggs were deposited in the larger cells, however, if no normal-sized cells were available. In hives having only the largest cells there was a tendency for the drone brood to increase as the comb increased in age; this is another indication of an inherent preference for cells of normal size.

It is evident that in species such as the honeybee the female, after mating, exhibits preferential oviposition; that is, the eggs deposited under one set of circumstances are fertilized and those deposited under a different set are unfertilized. Fertilization is effected by a chain of simple reflexes involving the antennae, the nervous system, and the spermatheca.

If deposited unfertilized, the vast majority of eggs of the queen bee would become drones, regardless of the type of cell in which they were laid. The capacity of a queen to fertilize a million or more eggs and omit fertilization of a few, so that drones occur at the proper time to mate with the young queen, is a remarkable biological achievement. Since the macrotype hymenopterous egg is capable of completing its embryonic development within the mother, the mother to avoid injury must dispose of it before the larvae hatch. This is done either by absorption or by deposition. It is noteworthy that the yellow ring at the base of the ovary in old queens consists of the remnants of absorbed eggs.[5]

When a hive becomes overpopulated, most of the worker bees leave in a swarm, taking the old queen with them. Soon thereafter a young queen leaves the hive on her mating flight. Within a few hours after mating, the several millions of sperm received from one of the drones have migrated from the oviducts into the capsule of the spermatheca. There they lie quiescent until about to be used for fertilization, some remaining quiescent for more than four years. (The quiescent condition of stored sperm is observable in hymenopterous species possessing transparent sperm capsules.) Watson[10] transferred year-old sperm from the capsule of a mated queen to the oviduct of an unmated queen, which then deposited fertilized eggs.

The spermatheca in the queen bee (Fig. 1, *A*), as in females of other insects, consists of a sperm capsule for storing sperms, a spermathecal gland for producing sperm-activating secretion, and a sperm duct connecting the capsule with the oviduct. The spermathecal gland opens into the unobstructed sperm duct where it joins the capsule. For an egg to be fertilized as it passes outward through the oviduct, a few of the stored sperm must previously have been activated.

Activation of the Spermathecal Gland

Studies on the spermatheca of various Hymenoptera, such as *Tiphia* and *Calliephialtes*, as well as on the honeybee,[4] show that in such species the secretions of the spermathecal gland serve both to transport the sperm and to activate it. Whenever the spermathecal gland is dissected from a gravid queen, active sperms are found in the glandular ducts and tubules. The quiescence of the sperm in the capsule normally is an effect of the weak acidity of the fluid medium, resulting from the CO_2 given off by the sperms themselves.[9] Activation of the sperms at the opening of the capsule probably results from a change in the H-ion concentration at that point through an increase in volume of a weak alkaline secretion from the gland. (According to Fyg,[7] the blood of the honeybee is alkaline.)

The sperm-activating function of the secretions of the spermathecal gland can be demonstrated indirectly in the black scale parasite *Coccophagus ochraceus*. In this species the gland is extremely small in relation to the sperm, and the secretion apparently plays no part in the transport of sperm to the egg. When the mated female of *C. ochraceus* is afforded the opportunity to deposit her complement of ripe eggs in rapid succession (an opportunity that rarely occurs under natural conditions), the first one or two eggs of a clutch are fertilized and develop into females, whereas the rest of the sequence are unfertilized and develop into males. The fertilized eggs are deposited in the body cavity of the host—the female standing on top of the host and drilling down through the dorsum. The unfertilized eggs are deposited outside and under the host, the female standing beside the host and thrusting her ovipositor laterally beneath it. Since the female uses different oviposition postures in depositing fertilized and unfertilized eggs, it is possible for an observer to ascertain the sex of an egg before its deposition. The fact that only one or two of the dozen or more rapidly deposited eggs are fertilized indicates that the gland lacks the capacity to secrete fast enough for sperm activation to keep pace with egg deposition. The observer,

however, by forcefully prolonging the intervals between successive egg depositions can bring about the fertilization of the entire complement of eggs produced by this parasite. It is obvious that the prolonging of these intervals allows for the replenishment of the glands and permits the activation of sperms every time an egg is deposited. It is significant that when the spermatheca is depleted of glandular fluid the female reacts differently to its host (depositing eggs underneath it) than it does when the spermatheca is not so depleted (in this latter case depositing its eggs in the blood of the host). Apparently the secretive capacity of the spermathecal gland is the critical factor involved in the control of sex in *C. ochraceus*.

In certain other species the critical factor in the control of sex is the differential reaction of the gland to host-derived stimuli rather than the secretive capacity of the gland itself. In such species preferential oviposition regulates the activity of the gland and thus the sex of the deposited egg. This appears to be the case with such species as *Microplectron fuscipennis, Metaphycus helvolus, Scutellista cyanea, Tiphia popilliavora,* as well as with the honeybee.[2, 6, 9]

Hydrostatic Control of Spermathecal Valve

In species of Hymenoptera such as the honeybee and *Tiphia,* in which the spermathecal gland is capable of supplying an amount of fluid adequate for the activation of sperm regardless of the rate of egg deposition, the sperm duct contains a valve for use in regulating the discharge of the secretions and the activated sperm contained therein.

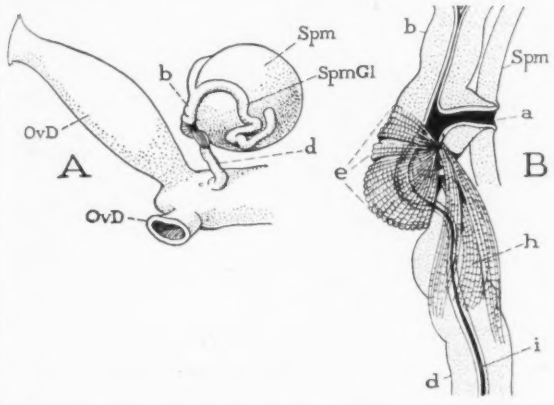

FIG. 1. (*A*) Spermatheca of the honeybee in relation to oviducts, and (*B*) the spermathecal valve, showing structure (from Breslau[1]): (*OvD*) lateral oviducts joining to form a common oviduct; (*Spm*) sperm capsule (estimated capacity, 4 million sperms); (*SpmGl*) spermathecal gland; (*a*) orifice of sperm capsule; (*b*) duct of gland; (*d*) sperm duct; (*e*) compressor muscles; (*h*) extensor muscles; (*i*) lumen of sperm duct.

This spermathecal valve (Fig. 1, *B*) is merely a bend in the sperm duct, resulting, perhaps, from the weight of the capsule and gland. The walls of the sperm duct at the bend, however, are supplied with muscles (Fig. 1, *e, h*) which operate to change the angle of the bend. The valve is closed when the walls of the lumen of the duct at the angle of the bend collapse.

The spermathecal gland pours its secretions into the sperm duct between the valve and sperm capsule. In both unmated and mated queens the secretions tend to accumulate back of the open valve, through which a small amount may flow into the sperm duct. According to H. H. Laidlaw, Jr., of the University of California, the gland and capsule lose turgidity through the escape of fluid when the sperm duct is severed above the valve, but retain it when the cut is made below the valve. In the latter case the capsule bursts if a high degree of pressure is applied.

When the queen is ovipositing at the rate of 2,000 fertilized eggs a day, the stimulation of the gland from impulses received from the antennae causes the development of a high hydrostatic pressure back of the sperm valve. This high pressure automatically tends to keep the valve closed. The fluid above the valve then becomes uniformly alkaline, obliterating the CO_2 gradients that direct the movement of sperms into the capsule. Under such conditions the sperms at the opening of the capsule move outward and permeate the fluid under hydrostatic pressure. While the sperm-filled secretion is under this high pressure, each egg as it passes the opening of the sperm duct causes the valve to open for an instant and emit a minute quantity of secretion containing several sperms, one of which penetrates the micropyle of the egg and unites with the egg nucleus.

When the queen oviposits in a group of drone cells, the hydrostatic pressure drops, the stimuli received when the queen inserts her head in the drone cell being insufficient to maintain high secretion by the spermathecal gland. This drop in pressure may not be immediate, since a few drone cells may receive fertilized eggs. With the cessation of external stimulation and the drop in hydrostatic pressure, the valve opens, the CO_2 gradients are re-established, and the unused sperms return to the sperm capsule.

Fertilization of the honeybee egg involves the following phenomena: (*a*) the storage of sperm in the queen; (*b*) the stimulation of sperm by the secretion of the spermathecal gland; (*c*) the construction by the worker bee of two sizes of brood

cells, the larger size being constructed in small groups among the more numerous, smaller-sized cells; (*d*) the facultative stimulation of the spermathecal gland, a conditioned reflex initiated by antennal contact with inner walls of the brood cells; (*e*) the capacity of the spermathecal gland to secrete enough fluid for transport of sperm, regardless of the rate of oviposition; (*f*) the closing of the sperm valve by a marked increase of hydrostatic pressure above it; and (*g*) the instantaneous opening and closing of the valve for ejection of sperm-containing fluid onto the micropyle of the egg as it passes along the oviduct.

It is through these correlating mechanisms that the sex and sex ratios of the normal hive population are subject to control. The basis of such control is the haploid production of males and the diploid production of females.

References

1. Breslau, E. *Zool. Anz.*, 1905–1906, **29**, 299.
2. Brunson, M. H. *Science*, 1937, **86**, 197.
3. Cohn, E. J. *Biol. Bull.*, 1918, **34**, 167.
4. Flanders, S. E. *Ent. Soc. Amer. Ann.*, 1939, 32, 11.
5. ———. *Ibid.*, 1942, **35**, 251.
6. ———. *Quart. Rev. Biol.*, 1946, **21**, 135.
7. Fyg, W. von. *Schweiz. Bienen Ztg.*, 1942, 65, 120.
8. Grout, R. A. *Iowa Agric. Exp. Sta. Res. Bull.*, 1937, **218**, 257.
9. Ullyett, G. C. *Proc. Roy. Soc.*, London, B, 1936, **120**, 253.
10. Watson, L. R. *4th Internatl. Cong. Ent., Trans.*, 1929, **2**, 976.

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Dam Site vs. Norm Site

BENTON MacKAYE

Benton MacKaye, who lives in Shirley Center, a small "wilderness" area not far from Boston, began his career in the U. S. Forest Service in 1905. In 1921 his deep interest in nature and in the conservation of natural resources found expression in his formulation of the project for the Appalachian Trail, the mountain footpath from Maine to Georgia. In 1945 he was elected president of the Wilderness Society and now holds the title of honorary president. In the pages immediately following his article we present also the plea of H. H. Bennett, chief of the Soil Conservation Service, for more effective use of our land and its potentialities.

WATER or wilderness? Reservoir or reservation? Which use of land, in any particular conflict, appears to contribute most toward the productive strength of the nation and the general welfare of its people?

This, in essence, is the question put recently to the Wilderness Society by the President's Water Resources Policy Commission. At least that is my interpretation of the question asked by the Commission's chairman, Morris L. Cooke, concerning how we can improve national water policy in the field of wilderness. The question could as well be turned around—how can we improve national wilderness policy in the field of water? For together they form a joint policy aimed at the *summum bonum.*

Seen thus, Mr. Cooke's question divides into two:

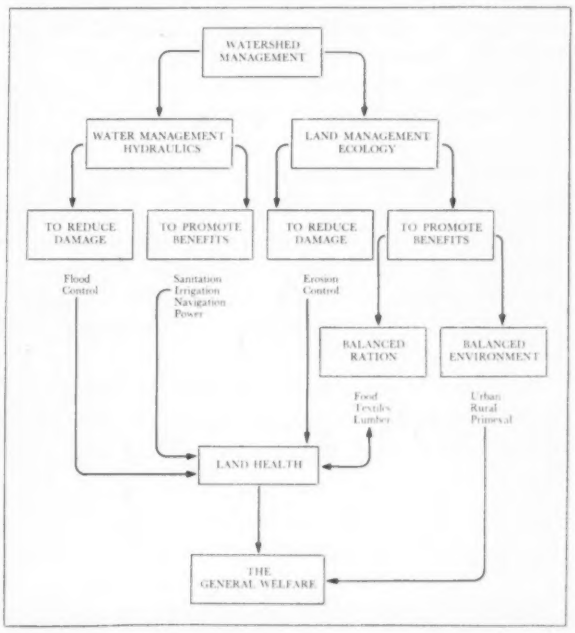
1. How can we compose conflicts between water and wilderness policies that arise from different means for attaining a common objective?

2. How can we compose the conflicts between these policies that arise from different immediate objectives?

Before attempting answers let us scan the field of action as shown in the accompanying chart. The total field is *watershed management.* This immediately divides into management of "water," or flow, and of "shed," or land.

Water management is based on the old science of hydraulics. Its job (so far as lies in man's power) is to control a flow of energy emanating from the sun. This flow, or cycle, is seen more concretely as a water chain: from cloud to rain to headwaters to river to sea to cloud, ad infinitum. The object of this control is to reduce damage, such as that from floods, while promoting a fourfold benefit (sanitation, irrigation, navigation, power production), all of which are working toward the common objective of land health.

Land management is based (or should be) on the new science of ecology. Its job also is to control a flow of energy emanating from the sun. This is evidenced concretely in a food chain: from soil to plant to plant-eater to carnivore to soil, ad infinitum. One object of this control is to reduce damage—such as excess erosion—while promoting the use of land for raising crops (food, textiles, lumber). Land health, of course, is basic to supplying this balanced threefold "ration" of food, clothing, and shelter. The other object, consistent with that of making a living, is to achieve and maintain a fit land wherein to do the living. This requires a balance, in terms of environment, as basic to human mental health as the balance in terms of ration is to human physical health. This environment is threefold—urban, rural, and primeval.



The primeval environment, or influence, is embodied in the untouched land, or wilderness, of which there are four main classes: forest, prairie, tundra, and desert. Wilderness is the antipode of city, and its primeval influence is as needful as the city's urban influence in preserving proper balance of the over-all environment.

How, then, shall we improve "water policy in the field of wilderness"—or vice versa?

We return to our dual question, in which two basically different functions of wilderness are referred to: (1) the wilderness as a physical resource, and (2) as a psychologic influence. Both are functions which, together with all water uses, are aimed at the same ultimate objective, "the general welfare," to "promote" which is the end job ordained by the American Constitution. But they seek this end by very different routes. As the chart shows, one route is by way of a balanced ration, the other, by way of a balanced environment; one via the daily individual necessities derivable from a normal healthy land, the other by the long-time regional amenities going with a land worth living in.

In other words, two distinct problems are involved in composing conflicts between water and wilderness uses: (1) where wilderness is a resource and an element of land productivity, and (2) where wilderness is an influence and an element of regional habitability. They comprise, in fine, two subject matters—one essentially material, the other essentially mental. They require, therefore, separate treatment; their present combined treatment is one source of the misunderstanding and confusion concerning them. Even their clients are distinct—the producer and worker on the one hand, the consumer and dweller on the other. But in this article we shall limit ourselves to problem (1), leaving problem (2) to another time.

"Water policy," as well as "wilderness policy," needs dissection; water uses, as well as wilderness uses, consist of more than one. Wilderness, as we have just seen, functions as two things (1) resource and (2) influence. Water functions as four things—(*a*) sanitation, (*b*) irrigation, (*c*) navigation, and (*d*) power production.

Here are eight possible conflicts in the general conflict of wilderness uses vs. water uses. Several of them have figured bizarrely in the nation's press. There is the historic case of function (2) vs. function (*a*), of wilderness influence vs. water for sanitation—the Hetch-Hetchy case in California around 1910. There is the current case of function (2) vs. function (*b*), of wilderness influence vs. water for irrigation, in Dinosaur National Monument, Colorado. There is the case of function (1) vs. function (*b*), of a wilderness wildlife area vs. an irrigation reservoir, in Glacier National Park, Montana. There are other combinations.

Of these, there is one that keeps recurring, that of the wilderness wildlife area vs. the irrigation reservoir. This illustrates most precisely the general conflict where the objective is the same but the means differ. This article will be concerned only with this particular class of competition. We shall not consider the Glacier Park case nor any other actual instance. Our aim will be to develop some common code of communication in the appraisal of alternative uses for a given piece of land. This can only be done by eliminating all extraneous matter. Every actual conflict includes such matter —whether legal, jurisdictional, traditional, emergent, or other.

We go back, then, to our original question, the one put to the Wilderness Society by the President's Commission—how can we improve national water policy (or wilderness policy) in the field of wilderness (or water)? Specifically, how can we improve relations between wilderness *resource* policy and water *irrigation* policy? The objective of each is the same; namely, the maintenance of healthy land. But the means differ. Water keeps land healthy by supplying it with needed matter; wilderness would keep land healthy by supplying it with needed knowledge and experience.

To see how this is so requires that we come to some agreement on the essential nature of, first of all, land. Is it organic or inorganic? "Organic" implies life. Is land alive or is it dead? The first lands must have been dead. Not till the Silurian age is life, other than microscopic organisms such as bacteria, supposed to have come upon the continents. Previous continents, therefore, were practically lifeless. Cambrian land, for instance, was essentially inorganic.

But something happened. Life forms occurred on the Silurian seashores; life found out how to make the change from a liquid sphere to a gaseous sphere, learning thus to occupy a solid sphere—land. Upon this land, by Carboniferous time, organic life learned how to spread a coating thick enough to hold and nourish huge marshes of giant forest growth; these marshes it is, with other terrains, that evolution has developed, now in Recent time, into the varied and manifold landscapes that we know today. So Cambrian land and Recent land are different. Cambrian land was essentially inorganic. How about Recent land?

The answer requires agreement on the precise nature of organic. Here is the statement of it in the words of the geologist Nathaniel Southgate Shaler: *It is the capacity of organic form to gather and store experience which demarks it from the inorganic.* What things in nature have this capacity? Not the vacant, lifeless continents of the Cambrian age. Not the present solid crust of the planet earth. Not the outcrops of this crust that show their rocky noses here and there within the Recent landscape. But what about all else that constitutes that landscape? By whom or what was it produced in all its present varied forms? Surely man has had a very recent hand in it. But what fashioned the American forest and prairie of A.D. 1492, step by step through all the ages, out of once-lifeless areas? Was it animal alone, or plant alone, or the two together? Was it by competing, or by cooperating? Were the two processes working in the treetops merely and above the surface, or were they working also among the roots and deep down in the soil? Apparently, everything had a hand in it—animal, plant, and soil—but do their processes constitute "experience"?

Again we must come to some agreement as to what we are talking about. What, precisely, is experience? The *Century Dictionary* gives six meanings. I shall quote the one labeled "specifically":

> *Specifically*—**3.** That which has been learned, suffered, or done; . . . the sum of practical wisdom taught by all the events, vicissitudes, and observations of one's life.

Who or what among all the elements of Recent "land" (outside of outcrops) has not, in its long work throughout geologic time, "learned, or done," if not "suffered"? What among them has not gathered and stored a "sum of practical wisdom" taught by a multitude of "events, vicissitudes, and observations"? Unless we can find some elements that have not so "gathered and stored," then we must conclude that all present land (above bedrock) is "organic form."

Whatever is organic must contain organs. Those of organic land occur, roughly, in three classes—animal, plant, and soil. Together they form an organism. This land organism was brilliantly described—both its structure and its means of functioning—by the late Aldo Leopold in *A Sand County Almanac* (New York: Oxford Univ. Press, 1949). The structure, or anatomy, he pictures as a "land pyramid" consisting of layers. From the top downward they are:

> The "apex layer," consisting of the "larger carnivores."
> The "bird and rodent" layer.
> The "insect layer."
> The "plant layer."
> The "bottom layer," soil.

The means of functioning, or physiology, consists of a successive eating process. Again proceeding downward:

> Every carnivore consumes "hundreds" of prey, which in turn consume:
> "Thousands" of prey (in the "bird and rodent" layer).
> "Millions" of insects.
> "Uncountable plants."
> "Plants absorb energy from the sun" (directly through the leaves, indirectly through the soil).

"This energy flows through a circuit," the land pyramid, or "biota," or "food chain." Each species, including man, is a link in this chain. A typical food chain of North America, 1492, was the "soil-oak-deer-Indian" chain. This has now been largely converted to a "soil-corn-cow-farmer" chain. (This happens when wild land becomes domesticated.) "Land, then, is not merely soil; it is a fountain of energy flowing through a circuit of soils, plants, and animals."

The essence, therefore, of Leopold's notion of land may be described as a particular circuit of energy. Land in this sense has not always been. Cambrian land could not be thus described. It dates apparently from the Silurian only. As Leopold says, "in the beginning," the pyramid of life was "low and squat; the food chains short and simple. Evolution has added layer after layer, link after link." To do this, evolution has been obliged, in Shaler's words, "to gather and store experience."

What is the greatest lesson learned from this experience? According to Leopold, it is health. "The most important characteristic of an organism," he says, "is that capacity of internal self-renewal known as health." Its other name is normality. And evolution has made a wonderful success of it. The capacity to gather and store experience has been successful in achieving that other "capacity" of internal self-renewal.

In other words, nature has shown us the secret of land health. Primeval organic land, the aboriginal food chain, such as a soil-oak-deer-panther food chain, possessed, we may say, perfect internal self-renewal, absolute health, 100 per cent normality. We cannot often say that of the soil-corn-cow-farmer food chain. Such is the difference between wild or primeval land, on the one hand, and tamed or domesticated land, on the other. One is a norm, the other a subnorm. The whole object of conservation is, of course, to emulate nature's successes; *this is not to make tamed land wild, but to make subnormal land normal.*

But, says Leopold, "practices we now call conservation are, to a large extent, local alleviations of biotic pain. They are necessary, but they must not be confused with cures. The art of land doctoring is being pursued with vigor, but the science of land health is yet to be born." Put in other words, what Leopold seems to mean is that land medicine and surgery are being practiced, but not land hygiene. We may or may not agree with this judgment, but surely a land hygiene, born or unborn, is basic to any imaginable conservation of natural resources.

Whether a true land hygiene (or "science of land health") yet exists, something at least akin to it has made its debut in the science called ecology. Its subject matter appears to be identical with that described by Leopold—the soil-plant-animal food chain. But "community" is what it is usually called. Ecology is defined as the "study" of this community, and one manifest purpose would be to keep its subject healthy. If study for this purpose be called *applied ecology,* we have a name at least—and a desire—for Leopold's conception.

But, semantics aside, one obvious need in any effort to render normal the subnormal is an exhibit of the normal. Imagine human hygiene without a normal human here and there, or enough near-norms from which to plot the perfect. Imagine the veterinary without an animal norm to test his treatments. As the horse doctor needs a normal piece of horseflesh, so the land doctor needs a normal piece of land area. Each of these organic norms —man, beast, land—is a body of stored experience. Each is needed as a base or standard or optimum to test treatment of the average.

Here is what Leopold has to say upon the matter:

> A science of land health needs, first of all, a base datum of normality, a picture of how healthy land maintains itself as an organism. . . .
>
> [The] most perfect norm is wilderness. Paleontology offers abundant evidence that wilderness maintained itself for immensely long periods; that its component species were rarely lost, neither did they get out of hand; that weather and water built soil as fast as or faster than it was carried away. Wilderness, then, assumes unexpected importance as a laboratory for the study of land-health.

Wilderness is the "perfect norm;" it is wild or untouched land as distinguished from domesticated, such as cornfield, pasture, or farm woodlot; it is the product of paleontology. *Wilderness is a reservoir of stored experience in the ways of life before man.*

This reservoir once covered all the continents. In North America it lay, roughly speaking, in four main divisions: the *tundra,* from Hudson Bay northward; the *prairie,* a long slice in midcontinent from Lake Winnipeg to the Rio Grande; the *desert,* in the southwest, in Mexico and along the United States border; the *forest* (northern, eastern, western, tropical).

Looking more closely, we find that within the United States this ecologic resource was divided into a number of ecologic provinces. They were all in the Temperate Zone; the tundra and subarctic provinces were absent except along the Western ridgetops and a few peaks in the East; tropical land was limited to the tip of Florida, from Lake Okeechobee southward.

Within this temperate belt, the land organism, by gathering and storing experience, learned to adapt itself to conditions of moisture (these resulting, in turn, from topographic form and distance from the oceans). Fairly thick forests grew on the humid Eastern half of the country, and thicker and higher ones still in the belt of extreme rainfall on the Pacific coast. Forests grew feebly on the less-watered slopes of the Rocky Mountains, but not in the dry Central Plains except along the riverbanks. Moisture-conserving vegetation such as cactus was all the land organism could produce in the arid Southwest. But between the Eastern forests and the Rocky Mountains, between the damp zones and the dry, it found a way to develop a lush growth of prairie grass, tall toward the humid East, short toward the semiarid West.

In the Eastern half of the present United States, the land organism, responding to a wide variety of local environments (climatic and topographic), became capable of developing a wide variety of local food chains. Trees were the most conspicuous members of these communities. Oak, hickory, chestnut, and tulip poplar formed the aristocracy of the salubrious middle belt from the Mississippi to Chesapeake Bay. White pine, spruce, maple, and birch were the "Who's Who" in the north, from Minnesota to Maine, and southward in a long leg atop the Appalachian Mountain Range. White oak and yellow pine stood out on the Southern piedmont and the coastal plain, from Louisiana to the Carolinas, with cedar and cypress in the coastal marshes.

Here is a bird's-eye view of the web of ecologic provinces once covering this country. They are the product of a vast primeval experience. Each province is a little ecologic world, an "ecologic civilization," we might even call it. Each is divided into lesser cultures, each with peculiar habits and customs. After 1492, these primeval civilizations

were invaded by one of very different hue, and the provinces were reduced to scattered remnants of their former glory.

Agriculture thus came to replace primeval culture—as it should. But agriculture divorced from primeval culture has lost its roots. It has not actually been divorced, but there has been an inevitable separation. Normal food chains have been replaced by revised food chains. A necessary shift has been made, for instance, from chain (*a*), to chain (*b*), from soil-oak-deer-panther, to soil-corn-cow-farmer. But if a farmer never sees a panther, say, he savors of a horse doctor who never sees a horse. If an agricultural practice such as (*b*) is never compared with the corresponding normal primeval process (*a*), it is like veterinary practice never compared with the normal equine process.

I say "if." I do not say that agricultural practice never is, in fact, compared with the normal process. It must be or else we should starve. But is it compared enough? Enough to understand the delicate balances involved in any food chain? Enough to understand the elements that affect these balances? Enough, for instance, to prevent or slacken the accelerating march of pests on crop and tree? Assuming that we do not enough compare the revised chain with the normal, it seems to follow that we shall lose the only opportunity now left to make the comparison.

This calls for Leopold's "system of wilderness study areas." It should comprise representatives of every important ecologic culture of every ecologic province wherein we intend to practice farming. For, as Leopold says, "One cannot study the physiology of Montana in the Amazon; each biotic province needs its own wilderness for comparative studies of used and unused land." A region or district without its ecologic representative would be like a Congressional District without a Congressman. (Indeed, the Congressman might well take this hint to heart—to see that his own district is ecologically as well as politically represented.)

Leopold goes on to say: "It is of course too late to salvage more than a lopsided system of wilderness study areas, and most of these remnants are far too small to retain their normality in all respects." If we cannot now have a complete system, then greater is the necessity to salvage what we can as soon as possible. These remnant exhibits, however, must suffice for Leopold's "base datum of normality;" they are the leftovers of history; there are only so many of them, particularly in any given ecologic province, and they are where they are. What kind of system of these exhibits can we visualize, which together would constitute a reservoir of experience in the normal processes of the land organism? Call it a system of ecologic knowledge. Before attempting to describe it, let us first visualize its counterpart, the water storage system needed to keep land healthy.

Let us say that land needs water as much as knowledge. Let us reduce the water problem to its simplest practicable terms: take one kind of water only; leave out water for power, navigation, urban supply; take irrigation water only. Generally speaking, in the interests of economy, two or more of these water uses should be combined, but for now, in the interests of comprehension, we shall keep to single rather than multiple purpose.

Consider one portion of the country only—the semiarid portion—and think of it as coinciding with a single extensive primary watershed, or river basin, all its meager rainfall draining finally through one main river into the sea. Assume its area to be that of the Colorado River basin, although it is not the Colorado River. Call it the XYZ River.

How much water can and should be stored in the XYZ River basin in order to irrigate as much of its irrigable acreage as needed by the nation for agricultural land? Suppose an estimate of so many million acres has been agreed upon as the quota for this basin. The average acre is found to need so much water per season. The total annual rainfall is so much per acre. From these data can be figured how many million acre-feet can and should be stored in the XYZ River basin.

The irrigation system consists of a series of reservoirs, each located above a convenient dam site. This system is based on a survey of the basin, followed by a plan or layout showing the dam sites in which the water can be most economically stored. Such is one kind of storage system needed to keep the basin's farmland healthy—the *irrigation water system.*

How about the other—*the ecologic knowledge system?*

We have assumed that land needs knowledge as much as water. Let us reduce this knowledge problem to its simplest practicable terms: take one kind of knowledge only, the one required to keep land healthy, one kind of knowledge and one kind of land—ecologic knowledge for agricultural land. Consider one portion of the country only—the semiarid basin of the XYZ River.

What knowledge, based on the ecologic experience now stored in the XYZ basin, would help to maintain healthy agricultural land within this

basin? Specifically, what data are available, from the basin's wilderness territory, concerning the processes of the normal food chains corresponding to the revised food chains comprising the basin's agricultural lands? The answers would determine what classes of ecologic data, and the extent of each, now available in wilderness lands, that can and should be reserved within XYZ River basin.

The system of ecologic knowledge would consist of a series of wilderness areas, or reservoirs of ecologic experience. Each area would be an exhibit of normal ecologic processes. I shall call it a norm area, or *norm site*. This system would be based on a survey of the basin, followed by a plan or layout showing the still-available wilderness areas, or norm sites, in which the ecologic knowledge, already gathered and stored, can be most usefully applied. Such is the other kind of storage system that is needed to keep the basin's farmland healthy—the *ecologic knowledge system*.

Both systems are needed for the land health of XYZ basin—and for that of every other water basin and ecologic province. The water storage system in the United States is a fairly venerable institution. But the knowledge storage system is, in Leopold's words, "yet to be born." Nine times out of ten they would dwell in peace together, side by side. The tenth time, conflict might occur. Indeed, it does occur, though one system is still prenatal. It occurs in the all-too-familiar case when the same area is desired for water and for wildlife. A fight is then at hand: "reservoir vs. refuge," it is called; but its latent and more discerning appellation is *"dam site vs. norm site."*

Thus, one means for attaining healthy land clashes with another means to the same worthy end. What, then, must happen? Very often, like other conflicts in this world, the matter can be adjusted. In a well-surveyed river basin, more than one plan is usually made, so that any given dam site has one or more alternatives. An alternative norm site would usually be more difficult to find. In general, remnant wilderness spots are scarce; and one storing any particular class of experience would present a scarcity compounded. In addition, the alternative norm site presents a difference in *kind* as well as in degree, compared with the alternative dam site. The latter case contains the element of choice; the storage in question is yet to be located by act of man and according to his judgment. But the former case would hardly ever allow a choice; the storage is already located, by act of nature, and cannot be relocated.

The real conflict arises when neither kind of site has an alternative suitable to its own means for achieving the same end—land health. The question then is: Which means, in any particular instance, is most effective to the desired end? The final problem thus is how to measure "effectiveness."

Let us take an example. Assume, pro tem, that some way is found, at least in the simpler cases, for measuring effectiveness to a degree acceptable, if not satisfactory, to both contestants involved. Suppose a given site of XYZ River is desired by one party, A, for a dam site (d), and also desired by another party, B, for a norm site (n). By what procedure might we determine which means, (d) or (n), would have most effect on the land health of XYZ River basin? Let us assume the following:

Party A estimates that the elimination of (d) from his system of irrigation water storage needed for XYZ basin would decrease the system's effectiveness by 10 per cent. Party B estimates that the elimination of (n) from his system of ecologic knowledge storage needed for the basin would decrease the system's effectiveness by 5 per cent. Each estimate is made independently and submitted confidentially to a third party, C. C calls in A and B, submits A's estimate to B, and B's to A. All three discuss the estimates and come finally to accept them as being sufficiently accurate; each party, that is, agrees to abide by the figures of the other. We now could have a formula for determining which means, (d) or (n), should be applied toward the land health of the basin.

To start with, the two systems, each taken as a whole, have previously been assumed to be of equal value for the end in view. Also, the needed national agricultural acreage has been previously determined, and the quota required from XYZ basin has been figured accordingly. It is the acreage to be irrigated by A's system of stored water. Whichever system, then, that is impaired most by the elimination of any portion of it, in any instance, is the one to be given preference. Preference, therefore, would go to the means (dam site or norm site) which, if eliminated from its respective system (water or ecologic), would lower the system's effectiveness by the highest percentage.

In this instance, the loss of means (d) would impair its water system by 10 per cent; whereas the loss of means (n) would impair its ecologic system by only 5 per cent; hence (d) would have preference over (n). If the percentages were reversed, the preference would be also.

The usefulness of any such formula depends, of course, on the possibilities of improving the accu-

racy of the measurement of the respective percentages of effectiveness with regard to the two systems. This approach to the problem is a vague, groping, embryonic attempt to reach a solution based on some common ground where benefits of widely different characteristics may be measured in fair and comparable terms. In other words, the total effect on land health of any given norm site must be judged on an equal footing with the total effect on land health of its competing dam site. It is not so judged today. This is because the effect of the norm site is not as evident as that of the dam site. How to make it so is the job of ecology. This job is just beginning; ecology must catch up with hydraulics; the ecologist must catch up with the engineer.

The first step the ecologist should take is a step taken by the engineer long ago. The ecologist must do for every ecologic province in the country what the hydraulic engineer has done for every water basin in the country, that is, locate a system of sites needed for his purpose. The engineer now has ready, and has long had ready, his map of dam sites for every major river basin. Thus prepared, he can figure how "effective" will be any one of these dam sites on their collective might as storage of irrigation water.

The ecologist, to be equally prepared, must make ready his map of norm sites, or wilderness areas, for every major ecologic province. Then he, too, could figure how "effective" any one of his norm sites would be on their collective might as storage of ecologic knowledge. Not till the ecologist has mapped his system of norm sites will he be on a par with the engineer who has already mapped his system of dam sites. Then, and not until then, can ecologist and engineer begin to talk the same technical language; not until then can they have anything like a common currency of measurement. Now they are in the same position in matters of their common field, conservation, as their ancestors once were in matters of trade and commerce; they are in the stage of barter and bargain.

Indeed, the condition is, unhappily, true of conservationists in general. They have no conservation "currency." They bargain instead of measure; they swap a dam for a tract of muskeg, much as their forebears swapped a goat for a barrel of rum. Not yet, of course, have men developed an accurate currency, even in matters of finance itself. The "dollar" is a made-up thing, a fictitious yardstick made of rubber; its notorious fluctuations expose its absurdity as a standard of real value. But even a defective currency is better than a barter system; it is at least an attempt at the correct measurement of the "effectiveness" of different kinds of things toward one and the same kind of objective. It is in a higher plane of thinking. The fundamental project for the conservationist is to develop somehow something, even if defective, within this higher plane.

The method herein described is an effort toward that "something"—an attempt, crude and groping though it may be, toward formulating some manner of currency or common language, an "Esperanto," for us poor tribes of wilderness and water. The alternative is the present-day babel. And so, at this stage of development, I have left out actual cases; I have omitted application of an Esperanto to current controversies. These are here and will continue. The first need in controversy is agreement, agreement on certain basic meanings of the nature of nature herself, meanings expressed in specific, definite symbols shorn of all irrelevant contentious associations.

This article, as we said in the beginning, attempts to answer only half of the question put to the Wilderness Society by the President's Water Resources Commission—"how to improve water policy in the field of wilderness?" Each half involves a comparison and appraisal of values. One appraisal takes the ecologic form—the comparison of water vs. wilderness as primeval experience, of dam site vs. norm site. The other takes the environmental form—the comparison of water vs. wilderness as primeval influence, of dam site vs. influence site. Primeval influence is increasingly demanded as the antidote for urban influence and of the environment of a highly wrought mechanization.

The formula described could be adapted to the environmental appraisal of wilderness, at least in principle. The practice in any particular case would depend, as with the ecologic formula, upon developing accuracy in measuring effectiveness; such development in the environmental case would involve human and social factors absent in the ecologic.

Wilderness is two things—fact and feeling. It is a fund of knowledge and a spring of influence. It is the ultimate source of health—terrestrial and human. In this article we have been concerned with one form of such health, with only casual reference to the other. But the necessity for conserving both of them must finally be recognized, and on equal terms appraised, if ever water policy, or any other policy, is to be improved in the field of wilderness.

Technical Skills for Soil and Water Conservation

H. H. BENNETT

Illustrations by photographers of the U. S. Soil Conservation Service

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FOR the first time in history, conservation of land, water, forest, grass, cultivated crops, and wildlife are being scientifically coordinated on the basis of land capability and need. I believe I can say that the national policy and public thinking, with few exceptions, are agreed on the utter necessity of conserving these interlocking natural resources. Research, education, surveys, and the successful application of conservation measures have brought us to a new concept of the importance of land and of the need for keeping the land permanently productive. No longer do we, in our thinking, planning, and action (work on the land), put farmland erosion off in one pigeonhole by itself, forest depletion in another, or floods and siltation under one heading and wildlife depletion under still another.

These are related things. Nature's laws were so contrived that land, water, plants, and animals should—and under natural conditions do—exist in harmony and interdependence for perpetual productiveness. It is a like mandate of nature that the land must be treated and used according to capability and need if it is to endure. This law of nature man too often has overlooked while busy with his daily affairs, or has entirely failed to understand because of confusion, war, and greed. The practical application of this principle is being demonstrated today on a progressively wider scale from year to year all over the nation, through the nation's soil and water conservation program.

The program of the Soil Conservation Service was developed out of a balanced relationship that exists in nature among land, water, plants, animals, and climate. Productive land, with the water that enables it to produce, is the one basic resource without which we cannot live. Without it there can be no life as we know it—except marine life—no forests, no food and fiber crops, no grass or other plant life. Recently we have even drawn on the life-giving soil for certain "miracle" drugs which science has developed as a further boon to mankind. Moreover, productive land is the source of the major part of all our wealth.

A large share of our scientific endeavor is directed in some way toward improving man's welfare—his health, his comfort, his wealth, or other needs or satisfactions in life. Scientists, therefore, are concerned about the land of the farmers' fields —although it was not always so. They are concerned, also, about our timberlands, our grasslands, and our marshlands. Except for fish, we are dependent on the land for the food we eat, for a very large share of our clothing, all our wood, and for many other things. Hence our most noteworthy scientific advancements in such matters as, for instance, crop and livestock improvement, or disease and pest controls, can be of limited worth at best if the productive land base of our plants and animals is not safeguarded.

Productive Land Unlike Other Resources

Productive land is unlike other natural resources such as mineral ores, coal, and oil. It is characterized by the element of life—fruitfulness —placed by nature in the thin mantle of productive soil occurring over a limited portion of the earth's surface. This life-producing quality, and the water that makes it so, in a sense, set land off in a category by itself. Productive land is further differentiated from other natural resources in that it must be used and maintained simultaneously. All other natural resources, with very few exceptions, must be taken from the earth—separated

from it—in order to be used by man. And their utility, in large degree, calls for complete transformation, as iron ore into steel, and coal and petroleum into warmth and power.

Productive land is much more limited than has commonly been supposed. It occurs only on the surface of the earth, and only on part of this surface. It is not permanent—it is not a renewable natural resource. It cannot be stockpiled or shipped in from other countries like rubber, tin, or copper. Once the fertile topsoil is washed or blown away, it cannot be restored or replaced in any practical way for generations. And what is left—subsoil—usually is far less productive, less stable, and less absorptive of rainfall. There are no substantial undiscovered reserves of productive land anywhere. And we cannot dig deeper into the earth and find new productive soil. We cannot pump it from wells, plant it with seeds, or dig it from mines. We must keep what we have or do without.

We occasionally hear loose discussions of the practical possibilities of remaking topsoil from raw, erosion-exposed subsoil. In my opinion it cannot be done short of geologic time. Subsoil can, in many instances, be improved, of course, by growing grass and legumes, for example, and by adding manure, compost, lime, fertilizers, and so on. Sometimes following such treatment good crop yields are obtained; but this is a matter of *improving* the subsoil, not of making new topsoil. I have spent a lifetime, as a practicing soil scientist, studying the soils over much of the United States and in other parts of the world. These studies support but one conclusion—that topsoil is one thing and subsoil is another.

Soil Conservation Requires Technical Skills

All of which brings us to the further premise that we must treat and use our remaining limited supply of productive land in a way that will at once protect it and increase its productivity by conserving the soil itself, its available elements of fertility, and all that man and nature have put into it. This conservation of our remaining land and water resources is the most important problem confronting American agriculture, and it is not a problem that can wait until farmers solve it by some trial-and-error method, for that might easily be too late. This is an urgent problem that demands the best efforts of the nation's scientists and of all who farm the land. It is a job that requires scientific knowledge, technical skill, and understanding cooperation.

It is true that many soil-conserving measures, especially annual farm practices, are so simple that farmers need no direct technical assistance in applying them. Putting lime or manure on the land, weed control, and growing protective cover crops are among those good farming measures that usually involve no particular difficulty and call for little or no technical help. But the principal soil conservation methods generally are used in combination with one another, and each must fit the land in a properly coordinated pattern to be effective and enduring. Based on painstaking scientific research and on wide practical use, such measures must conform in their application with the principles of hydrology, engineering, and agronomy. Under varying conditions of climate, topography, and drainage, the wrong thing done—or the right thing left undone—on any part of many farms can do serious injury to the entire farm.

Clearly, then, the over-all job of conservation involves such complex problems as erosion control, drainage, improvement of soil fertility, woodland management, control of running water, and wildlife conservation. But most farmers are not specialists in these fields, any more than they are their own doctors, lawyers, and dentists. Soil and water conservation is a job that demands the skill and knowledge of experienced technicians who have special training in the art and science. Experts trained in the specialized fields of agricultural science make up the personnel of the Soil Conservation Service, which makes virtually all its assistance to farmers available through the farmers' own soil conservation districts, and at their request.

At this point, I think it would be well to consider for a moment just what we mean when we talk about modern soil conservation. Modern soil conservation is based on sound land use and treatment of land with all the proved appropriate measures that will keep it permanently productive while in use. It means terracing land that needs terracing; and contouring, strip-cropping, and stubble-mulching the land as required, along with supporting practices of crop rotation, cover crops, green-manuring crops, etc., wherever needed. It means gully control, stabilizing water outlets, building farm ponds, locating farm roads and fences as nearly on the contour as practicable, planting steep, erodible land to grass or trees, development of good pastures, and devoting good management to them after they have been developed.

Modern soil conservation consists of doing all these and still other things. Where land is too wet, modern soil conservation calls for drainage, with full consideration given to wildlife needs; if it is too dry, it calls for irrigation; if it is subject to

wind erosion, it calls for wind-stripping, tree planting, growing cover crops, and stubble-mulch farming. If plant nutrients, lime, and the humus supply of the soil have been depleted, it calls for fertilization and addition of lime and organic matter; if water-soluble salts have accumulated in toxic quantities, as often happens in parts of the West, it calls for drainage and leaching out of the salts by flooding. Modern soil conservation calls also for the use of the best of the most adaptable varieties of crops, as well as the most efficient of adaptable tools available to farmers.

It likewise includes, for flood control and reservoir protection, treatment of whole watersheds with the right combinations of practices, land use, and small structures in the smaller watersheds where floodwaters start. Applied at the right time and place, such watershed treatment saves soil and reduces flood and sedimentation damage, puts water in the soil for plant use and, where conditions are favorable, in ground-water reservoirs, and otherwise benefits general farm, industrial, and municipal water users.

And, of course, an indispensable part of modern soil conservation is a support program of research that will provide for the welfare of the land at all times and provide farmers with the advantages that progressive science can contribute. Also, a continuing, vigorous program of soil conservation education should be made a part of our teaching, probably from kindergarten on through college. Moreover, modern soil conservation calls for the continued maintenance of all effective work which is put on the land, not just for a single year or cropping season.

In short, there is only one correct formula for doing the soil and water conservation job —a formula consisting of treating the different kinds of land on a farm according to their individual need or condition, and using each kind according to its capability for continued safe and economical production.

There are three basic steps in getting soil conservation properly applied to the land. First is the making of a scientific land inventory, which we call the "land capability survey." This inventory is made by Soil Conservation Service conservation surveyors, in cooperation with farmers, and it covers entire farms.* It shows the kind of soil, the slope, degree of erosion, and other factors which, together with climate, govern the whole safe use and producing capacity of land. This information, in the hands of landholder and technician, serves as an accurate land base for the development of scientifically accurate farm plans. It shows —field by field and area by area—the condition of the land and its capability for production, whether for row crops, pasture, timber, or wildlife. Thus capability Classes I, II, and III represent cropland, valuable in the order named; Class IV is best for grass but can be cultivated occasionally; Classes V, VI, and VII are suited for pasture or timber; and Class VIII is best for wildlife and parks.

Next, the technician and the farmer, with a land capability map of the farm, go out on the land together and develop cooperatively a complete soil conservation plan for the entire farm. It is of the utmost importance that the technician get out on the land with the farmer; a worth-while farm plan cannot be made in an office or through the use of printed directions. This farm operating blueprint, although aimed first at the maximum protection of the land, takes into full account such economic needs of the farmer as the income he must have to support his family and operate at a profit, his market situation, and so on. The plan specifies the best use to be made of each part of the farm, year by year; the crop rotations; the building of needed farm ponds, terraces, and so on; the development of good pastures; the planting of trees for windbreaks or woodlots; and other things ranging from land clearing to removal of stone fences.

Finally, the soil and water conservation measures called for in the plan are applied to the land. As already pointed out, some of the conservation measures are the kind that the farmer can put into practice himself, with little further technical assistance, but generally the complete plan of coordinated conservation practices is of necessity so complex as to require expert technical help in putting it to work on the land at the proper place and in proper adjustment. The farmer provides all the materials and labor and maintains his structures and practices once they have been installed on the land. If technical help is needed on the maintenance job, that, too, is provided for in the arrangement between the soil conservation district and the Soil Conservation Service.

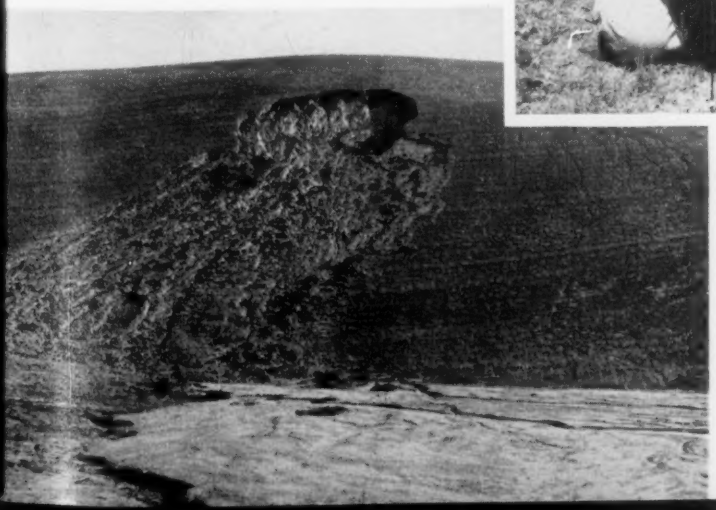
Each conservation measure is specifically designed to fit the land on which it is used. Furthermore, each measure is designed to support or complement another one, or several other measures, wherever support is needed, whether in the same

* This soil conservation survey was developed by the Soil Conservation Service. The first survey was made in the piedmont section by H. H. Bennett and Glen Fuller in 1935. (*Ga. Agric. Exp. Sta. Bull. 191,* May 1935.) The existing surveys pertaining to land cannot be used for soil planning.

George Follweiler farm, Stoney Run, Berks County, Pennsylvania. Contour peach orchard and diversion terrace.

Picture taken on the Boyer Nilson farm, five miles east of Troy, Idaho. Boyer Nilson's son is fishing from the north bank of the farm pond, which was constructed with equipment of the Latah Soil Conservation District, and stocked with fish by the Idaho State Fish and Game Department. The pond covers 1.78 acres and has a capacity of 4.47 acre-feet.

Height of plants on range cover evaluation plots is measured, and composition of the cover is determined by the "point-quadrant" method.



Both erosion and slides take place every year in the steep north-facing slopes of the Palouse wheat area.

Mark N. Witmer farm, Northumberland County, Pennsylvania. Ten acres each of peach and apple orchards next to woods. Strips: corn, oats, wheat, hay.

Fifty bushels of wheat are grown on winter wheat strip on Marion Holben strip-cropped farm near Genessee, Idaho. Background shows strips going across the slope of the hill. This layout of strips is farmed in a rotation of winter wheat, peas, barley, sweet clover and grass, sweet clover and grass.

This method of leaving a mulch of wheat stubble on the surface of the ground when planting the next crop of wheat is highly effective in controlling erosion in the Palouse country.

Land leveling for irrigation, using Eversman land leveler and Farmall Tractor, Blair, Oklahoma. Altus-Lugert Irrigation District.

On this Oklahoma field contouring and terracing are holding both the soil and all the water that falls on it. The water thus saved practically assures a crop.

Although this field in Brownsfield, Texas, is subject to severe wind erosion, it has never suffered badly, because sorghum stubble is always left standing as long as possible.

Soil carried by runoff from an eroding neighboring slope deposited on more productive land downstream, Anson County, North Carolina.

Ninety-eight yearling Pole Herefords on a bluegrass-orchard grass-lespedeza pasture mixture. Farm of H. S. Benson, two miles north of Owensville, Indiana. The grazing is so regulated that the grass-legume mixture provides adequate control against erosion and excessive runoff.

Contour strips on Heiden farm, Rushford, Minnesota.

George E. Lovell farm, Ririe, Idaho. Potatoes planted on the contour, and irrigated by means of gated surface pipe. First year under irrigation; field had been in dry farm wheat and fallow for some forty years.

Two-year-old grass-fat steers weighing approximately 1,300 pounds each. Amarillo (Texas) Conservation Experiment Station.

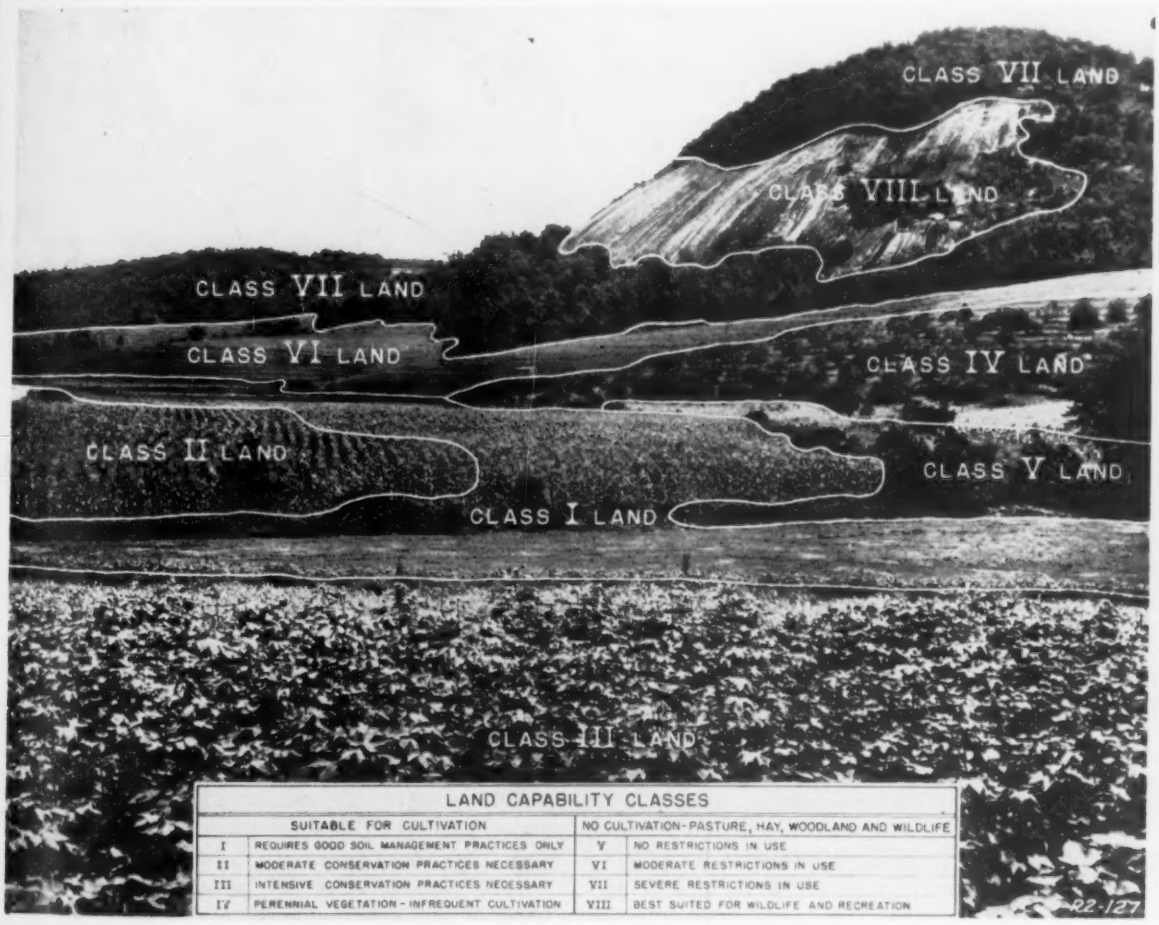
Severe erosion caused by a 3-inch, 30-minute rain in a Hockley County, Texas, field. Plowing was done up and down the hill, and at lower side of field along roadside ditch, three feet of soil and subsoil were cut away. The rain water also cut out deep grooves between every two rows.

field or in an adjoining field, above or below. Also, conservation work on one farm is planned and carried out with an eye to the needs of the next farm and to farms downstream. This viewpoint is essential, for the process of erosion has no respect for boundary lines. Gullies don't stop at fence lines, farm lines, or even county or state lines. Neither do dust storms nor floods. And the costly process of sedimentation never concerns itself with the interests of water users dependent on unprotected reservoirs.

Water Conservation

At this point, I want to say something about water conservation, a matter that has been of acute interest and concern to many of us during the past year. Drought, of course, had much to do with the much-publicized Eastern water shortage; but there are other matters, I think, that should be looked into, such as planning ahead for watershed protection for all new installations, and for remedying conditions, where necessary, on the watersheds of existing reservoirs.

In the Soil Conservation Service, when asked for suggestions, we always look first at the land within those watersheds where water shortages have occurred. We examine the condition of the land and how it is being used, from the standpoint not only of soil wastage but of water wastage, too. We look at all the land to see if any of it is being seriously affected by erosion, such as is most commonly caused in humid areas by (1) cultivating excessively steep land without adequate protection, (2) overgrazing, and (3) burning. All these abusive practices contribute to wastage of both water and soil. It is the excessively rapid runoff of water that produces erosion. And, what is of great importance in reservoir maintenance, is the prevention of filling up with the solid products of soil erosion. Washed-in soil takes up the valuable reser-


CLASS VII LAND
CLASS VIII LAND
CLASS VII LAND
CLASS VI LAND
CLASS IV LAND
CLASS II LAND
CLASS V LAND
CLASS I LAND
CLASS III LAND
LAND CAPABILITY CLASSES
SUITABLE FOR CULTIVATION
NO CULTIVATION-PASTURE, HAY, WOODLAND AND WILDLIFE
I REQUIRES GOOD SOIL MANAGEMENT PRACTICES ONLY
II MODERATE CONSERVATION PRACTICES NECESSARY
III INTENSIVE CONSERVATION PRACTICES NECESSARY
IV PERENNIAL VEGETATION - INFREQUENT CULTIVATION
V NO RESTRICTIONS IN USE
VI MODERATE RESTRICTIONS IN USE
VII SEVERE RESTRICTIONS IN USE
VIII BEST SUITED FOR WILDLIFE AND RECREATION
R2-127

This South Carolina piedmont farm shows the eight classes of land used by the Soil Conservation Service for making soil and water conservation farm plans. This method of mapping was devised by the Service and is being successfully used in its program throughout the country.

voir space needed for water. We cannot drink soil.

Much of the water that is prevented from immediate runoff soaks into the ground, partly to seep out below into our streams and reservoirs, partly to be available for plant growth, and so on. Water conservation, in short, is part and parcel of soil conservation everywhere.

More and more farmers are coming to realize that they need special technical help in planning and applying adequate soil and water conservation programs for their farms. This growing national consciousness of the essential place that such conservation has in our whole economy—a consciousness shared also by many and varied urban groups and interests—is evident in the great progress we have made as a nation in soil and water conservation within a comparatively few years.

The Soil Conservation Districts

These districts are organized by the farmers themselves under state legislation enacted by all forty-eight states, the territories of Alaska and Hawaii, and our insular possessions of Puerto Rico and the Virgin Islands. As of June 1, 1950, 2,247 of these farmer-managed districts already had been formed, embracing more than three fourths of the nation's farms and a total of more than 1,237,000,000 acres. Nine states are entirely covered by soil conservation districts: Alabama, Delaware, Massachusetts, Nebraska, New Hampshire, New Jersey, Rhode Island, South Carolina, and Vermont.

The soil conservation districts represent, I am convinced, the greatest land movement in all history. They are democratic units of state government. They are essentially group-action devices through which strong local leadership develops. They draw together, as at the hub of a wheel, the services and facilities of cooperating governmental and private interests alike, local, state, and national. And the soil conservation districts also are responsible for such tangible results as the creation of new markets for manufacturers of equipment and tools, and for the sale of the products of nurseries and seed producers.

A soil conservation district organization, of course, is not an end in itself. A district program that exists only on paper is not worth the paper it is written on. Neither is a farm plan for soil conservation where its provisions are not applied to the land. Up to January 1, 1950, approximately 800,000 complete soil conservation plans had been prepared in districts alone. These plans covered some 220 million acres, of which more than 112 million had been treated with needed conservation measures by that time. These figures do not include other millions of acres surveyed, planned, and treated through other programs in which the Soil Conservation Service has assisted.

Up to the first of this year, also, detailed soil conservation surveys adequate for farm planning had been made on 322 million acres. The primary objective of our broad program of conservation is, of course, to bring conservation treatment at the earliest practicable date to every parcel of land still needing it. And a primary requisite to achievement of such a goal is completion of the national land capability inventory at the earliest date facilities will permit. This scientific, acre-by-acre inventory of the land is the foundation on which rest effective conservation planning, treatment, and use of land for the permanency of the land, as well as the optimum development and maintenance of its water, timber, wildlife, and other land-related natural resources.

Increasing Pressure on the Land

The economy and efficiency of soil conservation farming, and the chain of economic and other benefits it brings, have been proved wherever it has been practiced. Thus, it enables the farmer to produce needed crops or livestock at minimum cost on his good land, and to adjust his production to changing market and other conditions. He therefore becomes a better and more stable customer for business and the professions in the city and contributes to the nation's high standard of living—nutritionally and otherwise—and to such public benefits as reducing flood and siltation damage.

Anything that helps any substantial number of individuals of a country also helps the country as a whole. This certainly can be said of the kind of soil conservation farming we have been discussing. We must keep our armed services and national defense machinery strongly built up, certainly; but we dare not neglect our domestic economy either. We do, after all, have to pay for ships and atomic bombs; and we can do this only by having an efficient economy, in agriculture as well as in industry and trade. And there can be, of course, no efficient agriculture without a base of good land.

Constantly increasing population and a still-shrinking supply of productive land will eventually make greater efficiency in food production more and more imperative. In addition, we may be moving along lines of development which will call for increased production of organic materials, such as organic plastics. That would mean additional pressure on the same land that has to produce our food and other essentials. These needs already are

exerting their influence on the world's limited supply of productive land and probably will continue to do so with such force as to challenge our utmost technological capacity. Every passing year demonstrates the growing importance of this relationship.

It is true that we have made remarkable progress in the field of soil and water conservation, but we need to move ahead still faster in order to get the big remaining job done in time. Lasting soil conservation is vitally important everywhere. It is mandatory here in America if we are to avoid eventual national decline in our economy. The margin between land deterioration and conservation has been narrowed within recent years. By increasing our present conservation operations on the land, we can—and should—get the principal basic measures of soil conservation applied to the land within twenty to twenty-five or thirty years.

Stop-watch measurements of the volume of water entering irrigation furrows make possible accurate determinations of the water intake rate of different soils under varying conditions of slope and soil composition.

We now know pretty well what the problem is, where it is, and how to solve it. We have perfected the tools of conservation—the methods and techniques and the knowledge for applying them with scientific reliability—and we have the farmers' soil conservation districts through which to work. We are moving ahead at a progressively faster rate year by year. In 1942, to illustrate, we completed the soil conservation job on 5 million acres; in 1949, the same thing was done on 22 million acres.

All we need now are additional facilities and the decision to go ahead fast enough to do the job on a scientific, permanent basis before it is too late—before we have wasted too much land. Unfortunately, we still are wasting land, even though we have learned that it is easier and more economical to conserve soil and water than it is to use those costly methods that impoverish land and waste rainfall as so much runoff to the sea. The more quickly the job is done, the easier it will be and the less it will cost.

More Understanding Cooperation Needed

We are faced with the important educational-technological task of spreading the understanding of this vital matter more widely and rapidly. It is a challenge to all of us, and each one has a part to play. From every conceivable angle—economic, social, cultural, public health, national defense—conservation of natural resources is an objective on which all must agree. The public and our society as a whole have a vital stake and, therefore, an enduring responsibility in these resources. It will take the best efforts of us all, working together on the conservation job, to get it done before it is too late.

The Elasticity of Soft Body Tissues

ALLEN L. KING and RICHARD W. LAWTON

Dr. King took his Ph.D. at the University of Rochester in 1937 and was formerly on its physics staff. At present professor of physics at Dartmouth, he also works as a consultant in biophysics and micromeritics. Dr. Lawton has an M.D. from Cornell University Medical College (1944) and is instructor in physiology there. The work reported was done at Dartmouth with the senior author and is being continued at Cornell under a grant from the U. S. Public Health Service. The present article is based on a paper presented before the Annual Meeting of the American Physical Society in New York City in 1949.

WHEN Robert Hooke,[1] that versatile experimentalist of the seventeenth century, enunciated his law of elasticity he wrote of it: "Nor is it observable in these bodies [i.e., springs], but in all other springy bodies whatsoever, whether Metal, Wood, Stones, baked Earths, Hair, Horns, Silk, Bones, Sinews, Glass and the like." Hooke was an expert anatomist, and frequently he was requested by the Royal Society to perform dissections. Probably he was the first experimentalist to investigate the elasticity of body tissues. It is notable that his free-ranging mind and clever hands were not bound to any one field of inquiry. Specialization was a foreign concept. About one hundred and thirty years later Thomas Young, whose name is forever linked with the subject of elasticity, delivered the Croonian lecture of 1808 "On the Functions of the Heart and Arteries."[2] He began his discourse with the words: "The mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies." It is not strange to find Young speaking on this subject, for he was a physician and could speak with some degree of authority. Again we find a man whose mind and hands were actively engaged in many diverse fields. It is in the spirit of these early investigators that we wish to present the recent advances in our understanding of the elasticity of soft body tissues.

At the time Young was carrying out his experiments in optics, one John Gough made the fundamental observations on the elastic properties of rubber. He communicated his findings by letter to Dr. Holme, of the Literary and Philosophical Society of Manchester, on November 16, 1802.[3] More than fifty years later Joule[4] repeated the experiments and added a few observations of his own. Meanwhile, in 1847, Wertheim[5] published an extensive series of measurements on the change in length of several body tissues with applied load. Only bone was found to satisfy Hooke's law with reasonable success. Other tissues, such as muscle, tendon, nerves, and blood vessels, displayed distinctly nonlinear characteristics. From the middle of the last century to the early part of this the problem of the elasticity of body tissues was studied as sporadically as that of rubber. Within the past twenty years interest in both problems has increased markedly. Rubber tubes occasionally are used as simple analogues of blood vessels, and rubber balloons sometimes are used to illustrate the elasticity of hollow viscera. Hand in hand with our improved understanding of rubberlike elasticity has come a better appreciation of the elastic nature of soft body tissues.

Let us examine some of the similarities of body tissues and rubber. In Figure 1 are gathered pressure-volume data for the rat lung, the turtle heart, the human bladder, and a toy rubber balloon. All show the same type of relationship and comparable ranges of relative extensibility. As seen in Figure 2 the load-strain curves for fibrin, collagen, and myosin are like that for a rubber band. Fibrin is the matrix of a blood clot, collagen is the principal substance in tendon, and myosin is the contractile element of muscle. Again the curves are strikingly similar.

The resemblance of natural tendon to stretched rubber and of heat-contracted tendon to unstretched rubber has been demonstrated in several ways.

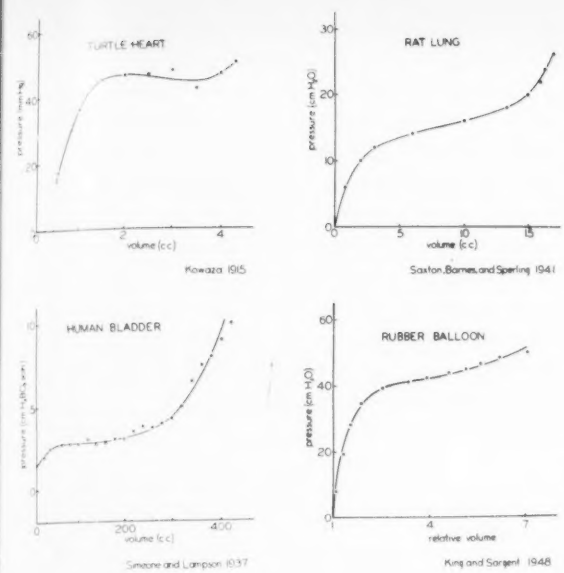


FIG. 1. Stress-strain characteristics.

Meyer[6] compared a stretched frozen piece of rubber with a specimen of natural tendon. Heat caused considerable contraction in both, of approximately equal amounts. Similar X-ray diffraction pictures reveal the presence of crystallite centers in natural tendon and stretched rubber. These centers seem to disappear on contraction. Birefringence studies and observations on fragmented frozen specimens are in accord.

The early work of Hill and Hartree,[7] later substantiated by such men as Wöhlisch,[8,9] Feng,[10] and Meyer and Picken,[11] showed that muscle has thermoelastic properties like those of rubber. For instance, in some observations by Feng it was found that for small loads the temperature rises on stretching, but for larger loads it falls on stretching. Apparently there exists an optimum load for which very little change in temperature occurs. Such an optimum load is found in the case of rubber (the inversion point). Meyer and Picken, on the other hand, made simultaneous measurements of force and temperature for the isometrically extended sartorius muscle of frogs. Their data show a relation between temperature and force similar to that for rubber. In general, the thermoelastic properties of muscle at rest are like those for rubber.

The preponderance of evidence from X-ray and birefringence studies, from observations on long-range extensibility, and from thermoelastic measurements suggests that soft body tissues are rubber-like. This thesis has been developed by Wöhlisch, Guth,[12] Meyer, and many other investigators. An adequate theoretical treatment of the elasticity of body tissues would be of tremendous aid in furthering our understanding of tissue functions.

The application of modern statistical theories of rubber to body tissues is fraught with difficulties. Tissues ordinarily are immersed in body fluids which permeate them. They are subject to relaxation and creep at relatively small extensions. Tissues are dynamic systems, undergoing growth and other changes continuously, so that statistical methods must be applied cautiously. Nevertheless, some success has been achieved.

In 1946 one of us (A. L. K.)[13] developed a theoretical pressure-volume relation for an ideal elastomeric tube and applied it to the human aorta. Some data of Hallock and Benson[14] on segments of human aortas in several age classes were replotted to show relative volumes of unit lengths as a function of pressure above atmospheric within the specimen. These new curves form a family which are well represented by the relation

$$P-P_0 = B\left[(V_0/V)^{1/3} \frac{L^{-1}[2\pi r_0 q (V/V_0)^{2/3}]}{L^{-1}(2\pi r_0 q)} (V_0/V) \right]$$

[L^{-1} represents the inverse Langevin function, where the Langevin function $L(u) = (u \coth u - 1)/u$; that is, if $L(u) = v$, then $u = L^{-1}(v)$], where V_0 is the volume of the vessel and r_0 is the radius when the internal pressure p equals atmospheric pressure p_0. The coefficient B is equal to $e_0 p_0/2r_0$ in which e_0 is the average thickness of the vessel wall. The q-factor characterizes the substance of the given tube, just as Young's modulus characterizes a substance satisfying Hooke's law. It has the dimensions

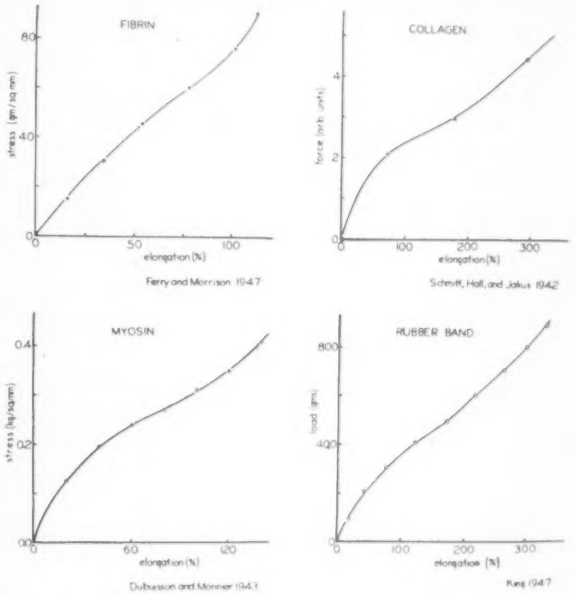


FIG. 2. Stress-strain characteristics.

of reciprocal length. This factor may be an index to the age of the aorta, for it appears to increase in value with age in an exponential manner according to the relation,

$$q = q_0(1 - e^{-a/a'}),$$

where a is the age of the aorta and a' is the time constant of the system. Probably q does not become vanishingly small in a continuous fashion as a approaches zero. The fact that a' comes out to equal twenty-one years in the case of human aortas may be purely fortuitous. Much more experimental work needs to be done to establish q as an index of age.

The cystometric data of Simeone and Lampson[15] on a normal human bladder can be described rather well by a relation developed for hollow elastomeric structures, as shown in Figure 1. In this relation there appears a dimensionless parameter β which equals the product of the circumference and the q-factor of the substance. The deviations at the upper end of the curve probably are due to the change in shape of the bladder within the body at large distensions and to uncontrollable variations when the subject becomes uncomfortable. For comparison we show in the same figure data obtained by King and Sargent on a rubber balloon for which the parameter β has the same value as for the bladder, namely, 0.42.

The cystometric data obtained by Langworthy, Reeves, and Tauber[16] on a cat's bladder to which the hypogastric nerves had been cut may be plotted as a family of curves. They show a decrease in bladder capacity and an increase in bladder "tone" with time after the operation. These curves correspond to a series of theoretical curves for hollow elastomeric shells for which β increases from 0.3 to 0.5. We are tempted to draw the tentative conclusion that over short time intervals changes in the characteristic parameter may represent changes in muscle "tone." Further observations and analysis may establish this parameter as a measure of "tone."

In this article we have sketched a little of the early history of tissue elasticity, we have described some of the rubberlike properties of body tissues, and we have shown some applications of modern analytic methods to these structures.[17] In doing so we have suggested an index for the aging of tissues and a measure of muscle "tone." We recognize that we have oversimplified a complex problem. We hope, however, that we may stimulate others to follow the lead of Hooke and Young in the study of the elasticity of soft body tissues.

References

1. Todhunter, I., and Pearson, K. *A History of the Theory of Elasticity.* Cambridge: University Press, 1886, 5.
2. Young, T. *Trans. Roy. Soc.,* London, 1808, 98, 164.
3. Gough, J. *Memoirs Lit. and Philosoph. Soc. Manchester,* 2nd ser., 1805, **1**, 288.
4. Joule, J. P. *Scientific Papers,* Vol. 1. Physical Society of London, 1884, 429.
5. Wertheim, M. G. *Ann. de Chimie et de Physique,* 3rd ser., 1847, **21**, 385.
6. Meyer, K. *High Polymers.* New York: Interscience Publ., 1942.
7. Hill, A. V., and Hartree, W. *Trans. Roy. Soc.,* London, B, 1937, **210**, 153.
8. Wöhlisch, E., and Clamann, H. G. *Z. Biol.,* 1931, **91**, 399.
9. Wöhlisch, E., and Grüning, W. *Pflügers Arch.,* 1942, **246**, 469.
10. Feng, T. P. *J. Physiol.,* 1932, **74**, 455.
11. Meyer, K., and Picken, L. E. R. *Proc. Roy. Soc.,* London, B, 1934, **124**, 29.
12. Guth, E. *Ann. N. Y. Acad. Sciences,* 1947, **47**, (6), 665.
13. King, A. L. *J. Applied Physics,* 1946, **17**, 510.
14. Hallock, D., and Benson, J. C. *J. Clin. Invest.,* 1937, **16**, 595.
15. Simeone, F. A., and Lampson, R. S. *Ann. Surg.,* 1937, **106**, 413.
16. Langworthy, O. R., Reeves, D. L., and Tauber, E. S. *Brain,* 1934, **57**, 266.
17. King, A. L., and Lawton, R. W. Elasticity of Body Tissues. In Otto Glasser, Ed. *Medical Physics,* Vol. II. Chicago: Year Book Publ., 1950. Pp. 303–316.

Three Poems *by* Daniel Smythe

THE BRAIN

In this grey temple of pulse and nerve
Are the vast steps by which we see
The flying sky, the tree's broad curve,
Or the horizon caught with sun
Or punctuated nights with starry glee.
These are the worlds the cerebellum has,
The lobes of hearing and of speech
Where the days pass
In slow, coordinated reach—
The pons, and hypothalamus,
And more of which we cannot know
Are here constricted. By these the shower
Of the tall land speaks to us,
And in the mind's eventful hour,
We learn and speak and go.

CONSCIOUSNESS

Out of a sleep I find myself awake
And pushing thoughts of senselessness aside.
The sights of land and trees are what I take—
And pain, and sensitivity and pride.

I am of life, and this a world that spells
The active atoms between moving darks.
My body carries millions of these cells
And senses that are bounded by two arcs.

The mind is conscious for a time to stay
In years hemmed by our ignorance; we tap
The reservoirs of sense in our brief day,
Joined to awareness that is like a trap.

SKYSCRAPERS

Incisively a mechanistic song
Probes buildings and redoubles tones of power.
They ricochet on waves that whirl along—
The human waves in streets and pointed tower.

The minds are guiding the wheels' thundering,
Hemmed by these walls of stone. I quickly sense
That deep on solid highways purls the spring
Of love forever dreamed, without pretense.

On all the heights, through penthouse, boulevard,
This that is life, assembled, drawn by hands,
Speaks of a passion that will not discard
The urge for something greater than steep lands.

What unchecked thirsts, what threads of hope we are
Who dared to make the crowns and pinions rise.
I almost hear them shout love's rising star
That towers are wearing through the rocky skies.

Evolutionary Determinism and the Fossil Record

GEORGE GAYLORD SIMPSON

A specialist in vertebrate paleontology, the author (Ph.D., Yale, 1926) has been on the staff of The American Museum of Natural History since 1927, chairman of the Department of Geology and Paleontology since 1944, leader of many of its expeditions, and professor of vertebrate paleontology at Columbia since 1945.

THE rise of modern science was characterized by the formulation of laws that were, or that seemed to be, absolutely deterministic. In the physical sciences, experimentation showed that defined circumstances produced predictable results with a certainty well within limits of observational error. These laws, such as the law of gravity or the law of combining weights, could be expressed mathematically. Their operation could be demonstrated by anyone and could be repeated as often as was desired. The variables involved were typically few, simple, and easily isolated. The passage of time did not prevent repetition of all the pertinent causal factors and produced no perturbation in the results. Gravity has no history.

The predictive value of such laws seemed to be 100 per cent. Although, in fact, the established laws of this sort covered only a small fraction of the phenomena of the universe, it was easy to conclude that the progress of research would discover other laws until the sway of mechanistic determinism would be found complete. Many agreed with Laplace that if we knew all about the universe at any given moment, we could predict exactly what would happen forever after.

Everyone knows that the physical scientists are no longer convinced of the universality of such absolute determinism. Indeterminacy has crept in on two different levels, which do not always seem to be clearly distinguished. Experimental verification and prediction may be impossible because the pertinent variables cannot be simultaneously observed. Apparent determinacy of events may be merely the extremely probable but not absolutely certain outcome of statistical effects, averaging very large numbers of individual cases which are not separately determined by the same laws. (That the individual events are therefore not strictly determinate by any laws whatever seems to be a conclusion of faith rather than of logic or evidence, but that is beside the present point.) This hedging on determinacy has considerable importance when delving into the atom or into the far reaches of the cosmos. It has also changed attitudes as to the basis for physical laws, but it has not really abrogated those laws. The gas laws may be merely statistical, but they continue to apply with full rigor to practicable observations in the laboratory.

The idea of a completely lawful universe was part of the intellectual atmosphere in which the theory of evolution was born and grew up. Evolution was commonly hoped or believed to be a process as orderly as, say, a chemical reaction. It was easy to assume that there must be a set of laws of evolution, absolute, universal, and deterministic, the discovery of which would provide a complete explanation of the history and present condition of life. If such laws have determined the history of life, they should be discoverable by examination of that history and especially of the fossil record. In this field, at least, the question thus becomes definitely one for scientific investigation and not one for philosophical speculation only. A considerable number of supposed laws of evolution were, indeed, promulgated by paleontologists and others; several of these "laws" will be named and considered later.

Some students would be willing to go all the way with Laplace and to hold that the laws of evolution are predictive, that the future history of life is (theoretically!) wholly implicit in the present moment. Probably most paleontologists, inclined to retrospection more than to prediction, would prefer to define absolute determinism as the belief that everything that has happened in the history of life had to happen; it could not have been otherwise. Evolution might then be likened to the working out of an equation with a unique solution.

Even within the fold of what can properly be called determinism, this absolute, Laplacian determinism does not exhaust the possibilities. Evolution might be *determined*—that is, completely caused in a materialistic way—and yet not rigidly *predetermined* from the first as to the course it was to follow. An equation can have multiple solutions, and yet each solution is determined by the equation. Then any laws, or rather determining principles, of evolution would fall short of full predictive value. The possibility of a nonpredictive determinism is enhanced by the fact that prediction in the physical sciences depends on the unrestricted repetition of causes. If causes cannot be precisely reproduced, then results cannot be precisely predicted.

Then, too, there is the possibility that evolution is partly deterministic, partly not. It may combine physicochemical determinacy with biological indeterminacy. This is the view of some vitalists, such as Vandel, for whom evolutionary determinism is a *loi approchée*. (In some current scientific circles it is considered old-fashioned or not quite nice to be a vitalist, so Vandel and some other vitalists do not accept this label on the package, but they supply pure vitalistic contents.)

The vitalists want to live in a dualistic universe with two essentially different sets of forces, principles, and essences: matter and the material forces, life and the vital forces. The finalists, who are usually also vitalists, want further to live in a purposeful universe. They want evolution to have had a goal, and they think that the history of life has been a means to an end, an effect preceding its cause as far as material manifestations occur. Both vitalists and finalists tread close on the heels of theology, for "life essence" may be soul, and the "purpose" in evolution may be God's.

In their bearing on determinism, vitalistic and finalistic views have involved a strange and almost surreptitious reversal of roles. Historically, materialism, mechanism, and determinism have been closely associated. To this day the three terms are often virtually synonymous in popular estimation and are likely to be considered as essentially associated by many scientists and philosophers. The curious reversal is that in evolutionary theory, the materialists have often tended toward indeterminism and the nonmaterialists (mostly vitalists, finalists, or both) toward determinism. These terms are not used and the issues have not been clear, but the confusion is largely semantic and the arguments are often mere obfuscations.

There have been two main currents of more or less materialistic evolutionary theory. One has run from Buffon, Lamarck, and St. Hilaire through neo-Lamarckism to Michurinism, further notice of which is deferred for a moment. The other has progressed from Darwinism through neo-Darwinism and mutationism to the modern synthesis (also often confusingly labeled neo-Darwinism). Proponents of these latter schools have made much of the partly or wholly random nature of what they take to be the materials of evolution: hereditary variation and mutation. Their opponents have accused them of believing in evolution by pure chance. This is not fully valid criticism because only a few extreme mutationists have considered evolution to be wholly random, but the point is that both the adherents and the critics of this current of evolutionary thought have emphasized its inclusion of random or chance elements in evolution. Although this need not rule out determinism of some sort or degree, it is hardly consistent with absolute, mechanistic determinism.

On the other hand, the vitalists, and especially the finalists (often but not always the same persons as the vitalists), have developed under other names a rigid determinism of their own. This is evident from the very fact that their basic criticism of materialistic theories is that the latter rely on chance altogether (which is not really true of most materialistic theories), or at least more than the evidence warrants. They speak of "antichance" as a peculiarly vitalistic or finalistic element in evolution, and it may be significant that the term is consciously borrowed from a physicist (Eddington). Cuénot, who among finalists is one of the few with a genuine, broad grasp of really pertinent evidence, recognizes both chance and antichance in evolution. He assigns chance to the mechanistic aspects of life, antichance to the finalistic, an unusually clear demonstration of the reversal of the usual associations.

All adherents of vitalist-finalist views adduce as primary evidence the supposed phenomena of orthogenesis, and define orthogenesis as a force (never really explained) impelling evolution to proceed only along fixed, predetermined lines. Osborn, another learned although somewhat obscurantist vitalist and finalist who certainly had tremendous knowledge of the evidence, held that "*fortuity is wanting*" in evolution. In this he spoke for many of the nonmaterialists. Surely a process without fortuity is deterministic, and it becomes clear that theirs is a theory of vitalistic determinism no less rigid than mechanistic determinism.

The role of Michurinism is also peculiar and should be briefly mentioned. It is ideologically improper for Soviet biological science to be other than materialistic and Darwinian. But Darwin and most

of his followers have not been wholly and rigidly mechanistically deterministic. Somehow materialism and determinism seem to be related concepts. From this sequence of conceptual associations, a dilemma arises for those who hold that science must be controlled by political Soviet ideology. The 1948 session of the Lenin Academy established Lysenko as high priest and political boss of Soviet biology and Michurinism as its gospel. One of Lysenko's clinching arguments was that the neo-Darwinians see nature as a "chaos of chance" and that Michurinism banishes chance. In other words, Michurinism is claimed to be deterministic, and this is promoted as an alternative to the indeterminacy of modern geneticists and others (mainly distinguished Soviet scientists who were genuinely materialistic and Darwinian).

This attempted escape from the dilemma involved in current concepts of determinism and materialism has plunged Lysenko and the Michurinists into a worse dilemma still, and it remains to be seen how soon this will become apparent to the Party bosses and what will be done about it. Michurinism is really vitalistic and not fully materialistic—on this we have, among other authorities, the interesting agreement of George Bernard Shaw, who approves of vitalism, and of H. J. Muller, who does not. Again the odd linking of vitalism and determinism appears. Moreover, Michurinism is a profoundly reactionary doctrine. It is largely pre-Darwinian and almost entirely non-Darwinian. Calling it "Soviet creative Darwinism" is such obvious double talk that it can hardly obscure the facts indefinitely. That Michurinism is reactionary, nonmaterialistic, and non-Darwinian has no conclusive bearing on whether it is true, but at least the strictly scientific issues are clarified by placing it in its real position among biological theories.

Let us now turn more directly to the evidence of the fossil record on the main problem. Does this record suggest a deterministic control of evolution and, if so, in what way and to what degree? The pertinent data consist largely of regularities constantly or repeatedly appearing in the record. Since these regularities, real or imagined, are summarized in generalizations read from (or into) the record by its students, our aim here may be brief examination of the validity and generality of some of the proposed laws and supposed tendencies.

It is hard to pin down the more rigid formulas of evolutionary determinism and to put them in concrete and testable form. Thus Berg, author of "Nomogenesis or Evolution Determined by Law" and among the most absolute of evolutionary determinists, has little to offer except the law that evolution is determined by law. The evidence put forward bears mainly on the claim that evolution has followed determined lines, that it has been orthogenetic. No satisfactory suggestions are made as to the nature of the determination, why one line was followed rather than another, or what the determinative instrumentality may be. Such vagueness or evasion is usual among the vitalistic and finalistic determinists, few of whom are so frank as Osborn, who maintained that the causes of evolution are quite unknown, although, in his opinion, the course is orthogenetic and directed toward a goal ("definite in the direction of future adaptation").

The crucial point here is whether evolution is in fact orthogenetic, whether orthogenesis is its law. There are many definitions of orthogenesis, but the pertinent one in this connection is that orthogenesis designates not only full determination of evolution but also rigid and unique predetermination. Orthogenetic evolution is supposed to proceed undeviatingly in a single direction, regardless of environment, organic activity, or such factors as natural selection. Discussion of this point has been so lengthy and extensive that it has, frankly, become boring. There is at present a clear consensus of paleontologists that orthogenesis, in this sense, is not real. There is no known sequence in the fossil record that requires or substantiates such a process. Many examples commonly cited, such as the evolution of the horse family or of sabertooth "tigers," can be readily shown to have been unintentionally falsified and not to be really orthogenetic. All supposed examples are more simply and fully interpreted as due to some other cause, such as natural selection. The fossil record is now usually cited in support of orthogenesis mainly by those least familiar with that record.*

The fossil record definitely does not accord with the particular sort of rigid determinism that has come to be associated with the concept of orthogenesis or more broadly with overtly or covertly nonmaterialistic theories like those of Driesch, Bergson, Osborn, Cuénot, du Noüy, or Vandel. There remains the possibility of materialistic determinism, which is a more open question.

A large number of evolutionary laws or principles have been proposed outside the field of orthogenesis and vitalism. As far as I know, no one has ever at-

* This summary dismissal of vitalistic orthogenesis is supported and warranted by the following review, which includes very extensive references to the pertinent literature: JEPSEN, G. L. Selection, "Orthogenesis," and the Fossil Record. *Proc. Amer. Phil. Soc.*, 1949, **93**, 479–500.

The general issue of vitalism versus materialism is discussed in: SIMPSON, G. G. *The Meaning of Evolution*. New Haven: Yale University Press, 1949.

tempted to compile them all, and few have attempted to select among them a consistent set that might be supposed to encompass the determination of evolution as a whole. Perhaps the most logical compilation is that by Sewertzoff,† who, with Vavilov, Dubinin, Schmalhausen, and others, is among the progressive Russian students of evolution whose work is anathema to the reactionaries now in control of the Party line in biology. His outline is brief and is worth reproducing (Table 1).

TABLE 1

	Types or Principles
I. The ancestral function of the evolving organ remains qualitatively the same in the descendants, but becomes intensified.	1. Intensification. 2. Substitution of organs, of N. Kleinenberg. 3. Physiological substitution, of D. M. Fedotow. 4. Fixation of phases. 5. Decrease in number of functions.
II. The ancestral function of the organ becomes qualitatively changed in the descendants.	1. Broadening of functions, of L. Plate. 2. Change of function, of A. Dohrn. 3. Similation of functions. 4. Activation of functions. 5. Immobilization. 6. Substitution of function. 7. Division of organs or functions.

This list of principles governing functional changes associated with morphological changes in evolution does not, in fact, reflect any regularity or necessity. It is a classification rather than a code, and its statements are descriptive rather than prescriptive. As far as there is any bearing on determinism, the list might suggest some degree of indeterminacy in evolution. All of Sewertzoff's "types" seem to be real phenomena and all can be illustrated from the fossil record, but they include alternatives or indeed tend to embrace all the possibilities. "Broadening of functions" is (with "no change" as an understood third possibility) the only alternative to "decrease in number of functions," "immobilization" is the opposite of "activation," and so on.

It is a "law" that, say, an evolving group of animals will increase in individual size or decrease or remain the same size, but this does not help to establish how or whether the outcome is determined. Such is the nature of Sewertzoff's principles and similar classifications of evolutionary phenomena. They are multilateral or all-embracing. Restrictive principles or laws are more likely to bear on problems of determination. As a matter of fact, one of the proposed restrictive "laws" of evolution is that evolving animals become larger. This sort of "law" is related to regularities in the fossil record, and it implies determined limitation of possibilities. A few of the more interesting of the supposed laws or principles of this limiting sort may now be considered:

1. Primitive forms survive longer than specialized forms (sometimes called "Cope's law of the survival of the unspecialized").

2. Body size increases in the course of evolution. (This has also been called "Cope's law," as well as "Depéret's law.")

3. At a given time, different evolving groups change in the same way (Dacqué's principle of *"Zeitsignaturen"*).

4. In the course of evolution, the number of structural parts becomes reduced and the remaining parts become more specialized (usually called "Williston's law," but Stromer thinks it should be "Stromer's law").

5. Evolution is cyclic, and evolving groups go through phases of youth, maturity, and old age (a "law" variously expressed by Haeckel, Schindewolf, and others).

6. Evolution is irreversible ("Dollo's law," but as in most cases the principle was known before its notice by the student for whom it is named).

Although these "laws" are here given in categorical form, most of their authors and discussants have qualified them, and qualification is necessary.

1. The survival of the unspecialized is certainly frequent in the fossil record. The opossum, little changed since the Cretaceous, has outlived many more specialized relatives. Numerous other examples are known. On the other hand, extinction of the less and survival of the more specialized seem to be about equally common. The living elephants, for example, are more specialized than the extinct mastodons, and in general all surviving ungulates are more specialized than many extinct allies.

2. Increase in body size is very common, a stock example being the change from eohippus to the modern horse. The phenomenon is perhaps sufficiently usual to be a rule, but the rule has many exceptions. Even in the horse family, several evolving lines became smaller rather than larger. The apparent extent of the rule has been exaggerated by students who thought it absolute and who insisted that because an earlier animal was larger than a later relative *therefore* it was not ancestral to the latter.

3. It has never been claimed that "program evolution," or *Zeitsignaturen,* tendencies for different lines to evolve in the same way at the same time, are universal. This has occurred, usually in the form of parallel evolution in more or less closely related lines, but as a matter of fact it is not so usual a phenomenon of evolution as to be considered a rule or the usual tendency.

† Sewertzoff, A. N. *Morphologische Gesetzmässigkeiten der Evolution.* Jena: Gustav Fischer, 1931.

4. "Williston's law" is well known to be an occasional tendency and not a universal principle. Its application is more limited than usually stated, because it works out only when an ancestral form has multiple parts performing essentially the same function, as in the dermal bones of the vertebrate skull or the legs of crustaceans. Under these particular conditions, reduction in number and increase in specialization of the remaining parts are clearly common and probably are usual. It is, however, well known that the opposite, multiplication of parts, is also a common evolutionary trend.

5. The idea that evolving groups have a phylogenetic life cycle has frequently been expressed, and numerous students have given names to phases of the supposed cycle. Three phases are usually recognized, but the names and descriptions of these phases vary widely among authors. A recent example is Schindewolf's distinction of "typogenesis," "typostasis," and "typolysis" as the three phases of (in his opinion) a universal and cyclic sequence in phylogeny. Phylogeny is certainly episodic, with evolution moving faster at some times than at others and with occasional periods of relatively rapid divergence ("explosive" evolution or adaptive radiation), commonly followed by slower progressive specialization of the various lines and by extinction of some of them. I can, however, find no good evidence that the episodic nature of evolution is truly cyclic in any regular way or that it has phases that can be called young, mature, and old in any reasonable sense of the words. The episodes of "youthful" divergence (typogenesis of Schindewolf) occur most irregularly and may recur within the history of a single group without intervening "old" stages. In the horse family, for instance, there have been at least three major episodes of this sort. The fossil record as I read it also lacks any valid evidence that lines of evolution reach old age in the sense of inherent waning of vital potential or that the claimed phenomena of "gerontism" are in any true sense indicative of terminal phases of a phylogenetic life cycle.

6. The principle of irreversibility in evolution is on a different footing from the others here listed. It is more general and more important for our subject. The principle frequently is not true of broad trends or of changes in particular structures and characters. A trend toward larger size, for instance, is readily reversible. A specialization, such as enlargement of a tooth or development of hoofs from claws, may also be reversed. A whole group may converge toward a remotely ancestral group, as whales toward fishes, and there will then be some reversion in adaptive characters. In these respects and within limitations as to exactness of duplication, evolution is reversible and the principle has misled students who thought that it applied to such cases. In a broader sense, however, the principle is true statistically if not absolutely. There are no known cases in which a structure, of any noteworthy complexity, at least, has reverted *precisely* to a distinctly different ancestral condition. With less qualification, there are no known cases in which a type of organism that had become extinct or considerably modified was ever reduplicated.

That evolution is irreversible is a special case of the fact that history does not repeat itself. The fossil record and the evolutionary sequences that it illustrates are historical in nature, and history is inherently irreversible. Realization of this fact puts the question of evolutionary laws and of determinism on quite a different basis.

Physical or mechanistic laws depend on the existence of an immediate set of conditions, usually in rather simple combinations, which can be repeated at will and which are adequate in themselves to determine a response or result. In any truly historical process, the determining conditions are far from simple and are not immediate or repetitive. Historical cause embraces the *totality* of preceding events. Such a cause can never be repeated, and it changes from instant to instant. Repetition of some factors still would not be a repetition of historical causation. The mere fact that similar conditions had occurred twice and not once would make an essential difference, and the materials or reagents (such as the sorts of existing organisms in the evolutionary sequence) would be sure to be different in some respect.

It is impossible to observe whether Cambrian trilobites would have evolved in the same way under the same conditions in the Ordovician. Cambrian trilobites did not exist in the Ordovician and could never again exist after the Cambrian. Nor were the conditions in which they evolved present in the Ordovician or at any time after the Cambrian.

Recurrence of more or less similar conditions within the great complexity of historical causation may evoke some similarities of response, but these will be flexible, nonidentical, and representative of tendencies rather than of invariable responses to law. "Laws" like those cited above clearly generalize such tendencies and are not laws in the usual mechanistic sense. At most they are descriptions of what has commonly happened, or in some cases only of what has sometimes happened. The fossil record is consistent with historical causation that is in continuous flux, nonrepetitive, and therefore essentially nonpredictive. It is not consistent with

absolute, repetitive, and predictive determinism.

History does not correspond with possible mechanistic models such as serve (with certain reservations, of course) in the physical sciences. That history is not simple and tidy is unfortunate, perhaps, but it is true. Probably all teachers of historical subjects have had students whose training or bent is in the physical sciences and who have become bewildered or antagonistic on learning that the important questions of history cannot be answered by an equation or an experiment. The human desire for neat and unequivocal conclusions explains the long and necessarily futile search for simple, absolute deterministic laws of evolution.

The record does show beyond any doubt that there are directional forces in evolution. There are trends and there are common tendencies. These are reflected in the "laws" that have been proclaimed from time to time, but those "laws" are not laws. The regulation, which seems surely to be a determination, is by forces that change continuously in intensity, direction, and combination, and that produce quite different results in different instances. These forces are themselves interwoven with the historical process and subject to historical causation, rather than being absolute or unchanging, in the sense of physicochemical laws, or yet internally inherent (as in vitalism) or metaphysically predetermined (as in finalism). Only two possible sorts of forces are consistent with these requirements of the record. One sort involves the neo-Lamarckian factor, the inheritance of changes directly induced by organism-environment reaction. This possibility has been thoroughly tested both by consideration of the fossil record and by experimentation, and it has necessarily been discarded, except in the untenable and retrogressive doctrine of Michurinism. It is inadequate to explain the regulation apparent in the fossil record, and it is experimentally disproved.

The other possibility is natural selection, and this must be accepted, not by elimination, but because it is entirely consistent with the fossil record, adequate to explain the regulation evident in that record, and experimentally verified. Like the neo-Lamarckian factor, formerly postulated but now known to be nonexistent, natural selection arises in the material historical process. It is also an interaction between organism and environment the results of which, in itself, are determinate, although the process of determination is extremely complex, far more so than if the neo-Lamarckian factor were really operative.

Beyond this point, natural selection is characterized by not producing its results directly. It can only act on materials presented for its action, which are genetic variations. Given an existing population structure and an existing ecological situation, and given the genetic variation of the population as it moves through time, the action of selection seems to be fully deterministic. The first two elements, population structure and ecological situation, are themselves entirely *historically* determined. Genetic variation apparently is not fully determined in the same way, or, rather, if it is so determined, we do not know how. As now known, both mutations and the genetic effects of sexual reproduction have a random element, in the sense that they have aspects for which historical causation cannot be assigned. These aspects do not seem to be consistently oriented with respect to the direction of natural selection. There is no good reason to doubt that genetic variations are materialistically caused, although rigorous proof is lacking. Yet the fact that they are in part random in relationship to the historical determinant of natural selection means that they make possible multiple solutions of evolutionary problems.

All this fits in perfectly with the known fossil record. Trends exist, but they are flexible and are not precisely repeated. Different results do follow similar causes or appear as solutions of the same adaptive problems. There are limitations, but they are not absolute. There is direction, but it wavers, and apparently random effects also occur.

(One more suggestion is thrown in, tentatively and parenthetically. As it affects some organisms, especially higher animals and most particularly man, historical causation may involve anticipation. Not only the totality of the past but also, at these levels, something of the future may thus be involved in evolutionary determinism. I do not, of course, refer to the metaphysical finalistic idea of a future goal which determines preceding events, but to the material fact that man and some animals sometimes act in anticipation of events that have not yet occurred. The anticipated events may or may not occur when the time comes. The possibility of multiple solutions and of nonpredictive determinism may thereby be enhanced. Anticipation is completely absent in physicochemical causation, and it adds to the reasons why evolutionary "laws" are not like physicochemical laws. Anticipation is nevertheless a fully materialistic process.)

Evolution is in part demonstrably deterministic. It is presumptively so in its entirety, but only in a special sense of determinism. The peculiarity of evolutionary determinism consists in its being historical and not mechanistic and in its permitting multiple solutions and not only a unique outcome. It is therefore both nonrepetitive and nonpredictive.

Can Science Transcend Culture?

JESSIE BERNARD

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THE psychological challenge to the claims of objectivity in science from the laboratory studies on the social rather than the physiological nature of perception has been paralleled by a similar challenge from the students of culture. Recent events have seemed to reinforce the belief that science is but a cultural creation, a tool, without claims to true objectivity. Until the twentieth century few people doubted that science, at least physical science, was supracultural, or at least pancultural, identical in all cultures. Even the Marxists, whose theories in effect denied the possibility of objectivity in science, arrogated to their system the adjective "scientific" in order to demonstrate its objective validity. Physical laws were the same in China as in Europe, astronomy was identical in Russia and in Africa. The natural laws were in the universe, so to speak, and men simply discovered them. Culture had nothing to do with them. Whoever heard of German botany or Protestant mathematics?* Botany and mathematics were above national and cultural boundaries. Science was an international entity, as "international as the north wind." People were amused at the Catholic Church's little skirmish with Galileo and its insistence that science conform to church doctrine. The concept of Catholic science was unthinkable. The accommodation of religion to science as represented by so-called Natural Theology and as embodied in the Bridgewater Treatises seemed much more the normal order of things than the reverse. The religion of nationalism had not yet demanded the obedience of science. To be sure, the Franco-Prussian War had created a breach between German and French scientists—the physiologists held no international meeting in either France or Germany until 1907 because of the survival of resentments from that war—but at the 1907 meetings science itself was declared to be independent of political frontiers and national jealousies.

Thus, when Hitler demanded that science be made to serve nationalistic ends, to most scientists the idea was horrifying, retrograde in the extreme. He banished Einsteinian physics because it was relativistic. Freudian psychology was Jewish and

* On a purely descriptive level sciences may, of course, be quite limited in scope. Thus, although a scientist might cringe at the notion of Russian astronomy, the idea of, let us say, Russian tundra botany might not disconcert him at all. For, obviously, the concrete data of science may vary from place to place. The biological and the earth sciences resemble physics and chemistry in the generality of their phenomena, but like all sciences that can use the survey method of securing data, they also resemble the social sciences; fauna, flora, and geological formations vary as much in different areas of the earth as institutions do. There are biological and geological facts, universally true, which can be formulated into scientific laws. Thus the processes of biological growth and decay can be described in laws of universal validity. Nevertheless, the botany of a jungle is different from the botany of the Russian tundra. In this sense there is an Amazonian botany, and a steppe botany. Similarly, although the geological processes that mold the earth are of universal enough scope to make possible general geological laws, the geology of any one part of the earth is different from that of another. In such a sense, earth and biological sciences are different in different parts of the world. We can legitimately speak of tropical botany because the biological processes operate in unique ways in the tropics. Similarly, in the case of the social sciences, we may say that there are certain phenomena of universal validity. Wherever people live in association, which, of course, is everywhere, certain phenomena are generated that can be described in general, universal laws. Organization, conflict, competition, are among these universal phenomena. As in the case of the earth and biological sciences, however, the actual distribution of cultural phenomena gives rise to rather specialized sciences. In such a sense we might speak of the sociology or economics of the United States, Russia, or China, as one would of the botany or geology of the United States, Russia, or China. We are not, in our discussion here, dealing with science in this descriptive aspect.

therefore must be proscribed. All science, even mathematics, had to be oriented toward German nationalistic goals. The degradation of anthropology to a genocidal weapon by the Nazis is one of the outstanding scientific scandals of the century. Because all this was so contrary to the nineteenth-century conceptions of science, most people felt it was a temporary aberration that would surely soon pass away. Yet today in the Soviet Union we are witnessing again the subservience of science to political ends, most spectacularly in genetics, but also in astronomy and the social sciences, especially psychology, economics, and sociology. Even in the West, which has nurtured the pancultural tradition of science, atomic research is jealously guarded and Joliot-Curie announces that "our accomplishments in the field of atomic energy are entirely French."

Science no longer, therefore, appears to be a majestic, impersonal, independent system operating the universe, which man discovers and to which he submits. When he appeals to it for help in the solution of intercultural problems, he finds it to be a human, social institution, with all the limitations of its creators. No wonder, then, that grave doubts about its true objectivity have come to haunt him. These facts, among others, make it worth while for us to re-examine the relationship between culture and science. This discussion undertakes this task and attempts to show that, although the direction of scientific inquiry may be culturally determined, the methods of pursuing scientific inquiry—scientific techniques—contain their own protection against cultural (and even personal, idiosyncratic, and perceptual) biases and therefore may rightfully claim objectivity for their results.

I

When we remember that science is itself a phase of culture, it becomes obvious that no science can develop unless the culture base is adequate. There can be no calculus until there is geometry, no statistics without calculus, no multidimensional geometry without plane geometry. In this elementary sense, the relationship between science and culture is the same as the relationship between any cultural invention and its base. Similarly, at the present time science is intimately dependent on instruments and machines of many kinds, ranging all the way from simple laboratory equipment and gadgets through tabulating machines to the fabulous calculating robots. The machines in turn react upon science, so that their mutual interdependence becomes almost organic. No science can soar beyond the physical equipment necessary to gather and process its data. That a culture must be rich enough to support the institution of science is another clear-cut fact. Scientific research is expensive; it is a luxury of wealthy cultures. Germany, we are told, in spite of theoretical knowledge, had to abandon efforts to create the atomic bomb because she lacked sufficient electric power.

The relationship between science and the nonmaterial aspects of culture is more subtle. "Patterns" of culture develop which reflect themselves in the laboratory as well as at the altar. The peculiar cultural slant to which even the physical sciences may be subject is illustrated by Emile Boutroux' evaluation of German science a generation ago. From the French point of view, "the Germans excelled rather in the mechanical parts of scientific work than in invention . . . their methods of explanation were frequently vague and obscure; . . . the practical applications of science in Germany were becoming increasingly more important than was disinterested investigation" (*Philosophy and War*). The German approach was well suited to gathering great volumes of data, but was it adequate, Boutroux asked, for the production of ideas? "The theory implied in the German method is that the idea is born by spontaneous generation from the materials themselves, once these latter have been conveniently collected and arranged." In spite of the German scientists' exaltation of competency, this French critic continued, "Can the man who deliberately eliminates from scientific research every living and human, personal and rational element, and retains only materially objective data and reasoning that excludes all intuition, be really competent in anything whatsoever?" By way of contrast, he went on, French science has followed the principles laid down by Descartes. *"Le bon sens"* is essential here. The object of Descartes' *Discourse on Method*

> is to prove that the same good sense governs both the practical life of the average man and the loftiest speculations of the mathematician, the physicist and the philosopher; that all science runs the risk of wandering astray unless, all along the line, it is constantly being controlled by good sense, and that this good sense, the link connecting our thought and reality, is the true source of invention and judgment, without which science is no more than an object of instruction and practical application. [Descartes adds that] good sense should be cultivated, and that the right means of developing it is reflection, fostered alike by the study of science and the experience of life.

Whether or not this Frenchman's evaluation of German and French science is correct, the fact that it was even made illustrates how cultural biases may stamp the very theory of scientific method itself.

When we turn to the conditioning effect of language on science we approach some of the subtlest of all relationships between culture and science. That a culture's language be capable of precision is a basic prerequisite for the development of science. Julian Huxley has made a graphic presentation of the fact that the kind of mind man has could not have occurred in any other animal than man. Human evolution as he pictures it was like a maze with far more blind alleys than clear paths. Hundreds of evolutionary changes ended in such blind alleys. The course of development which led to the human brain was dependent on numerous preparatory steps, all of which one by one eliminated every animal except man. By analogy we might say that the kind of institution which science is could not have occurred in any other than an Indo-European-type language culture. (Inventions like the Mayan calendar and gunpowder represent a somewhat different problem from the development of the institution of science.) Science as we know it is based on the kinds of language in which scientists have thought.

> We cut up and organize the spread and flow of events as we do largely because, through our mother tongue, we are parties to an agreement to do so, not because nature itself is segmented in exactly that way for all to see. Languages differ not only in how they build their sentences but in how they break down nature to secure the elements to put in those sentences. . . . As goes our segmentation of the face of nature, so goes our physics of the cosmos. (B. L. Whorf. *Techn. Rev.*, April 1941, 250–72.)

And, of course, all our other sciences as well. The author just quoted points out that other languages might furnish us with new logics for science.

> Western culture has made, through language, a provisional analysis of reality and, without correctives, holds resolutely to that analysis as final. The only correctives lie in all those other tongues which by aeons of independent evolution have arrived at different, but equally logical, provisional analyses. . . . An important field for the working out of new order systems, akin to, yet not identical with present mathematics, lies in more penetrating investigation than has yet been made of languages remote in type from our own.

Granting that other kinds of language have suggestive potentialities in new directions, it seems doubtful if science will ever transcend Western culture in the sense of reviewing all its achievements from the point of view of the logic of other languages. Nor is it likely that any other kind of language will ever contribute independently to science. For better or for worse, the Indo-European language mentality has determined the course of science, and when other cultures wish to incorporate science they borrow from this great bank. They coin terms for the concepts invented in this type of language and bend their grammar and logic to conform to the pattern already set. Thus, although science is perhaps permanently fixed within the mold of Western culture, other cultures can take it over.

There are even subtler ramifications of such cultural determinants of science. It was Karl Marx who first elaborated the point of view, now incorporated with variations into the culture concept, that men's ideas and even their sciences were intimately dependent upon the matrix in which they developed. The details of the two theories—one of class determinism, one of cultural determinism—vary somewhat, but the general context is similar. Marx undercut the entire concept of objectivity, as Freud did in a somewhat different sense some years later. Men thought the way they did because of their institutional background. Bourgeois ideology was an elaborate rationale of the bourgeois position in society. It could be no other. It was impossible for bourgeois social thinkers to perceive or to think in other than bourgeois terms. The value premises of their research must inevitably reflect their bourgeois point of view. Proletarian social thinking might be just as class-bound, but it represented the wave of the future, a majority of mankind, and was therefore a less specialized, less narrowly biased, more widely applicable, and hence a truer, point of view. The two were of necessity in conflict.

We have changed the terms for the ideological framework within which sciences develop. We now speak of cultural rather than of class matrices. But the problem remains. Our way of looking at the world, according to this theory, is determined for us by our culture as a whole, as well as by the one aspect of it we have already considered, our language. We have come to use the term "definition of the situation" to describe the process which people go through in perceiving, evaluating, and interpreting what goes on about them. Even within a given culture, among people using the same language, the student and the teacher, the husband and the wife, the parent and the child, the employer and the employee, define an identical situation quite differently. They all see, hear, and perceive different things, and what they see, hear, and perceive has different meanings for each. Nicholas Pastore, in *The Nature-Nurture Controversy,* has shown how even men of science whose integrity could in no sense be impugned frame their research problems and interpret their findings in accordance with basic preconceptions. If such differences exist within the same culture, how much greater are they among individuals

with different cultural backgrounds? A cannot make B even perceive certain phenomena if B's culture has not sensitized him to them. B will interpret them away. The Trobriand Islanders are thus culturally sensitized to be oblivious of the father's role in paternity. They insist on ignoring it. Research on this subject would doubtless be forbidden even if they had the institution of science. Peoples in some cultures see a big difference between parallel and cross cousins. We are oblivious of it. Out of the infinite variety of phenomena we in our culture select certain ones to see. Communists, by way of contrast, are sensitized by their culture to select others. Two people with different cultural backgrounds observe identical phenomena. One reports "democracy," the other "dictatorship;" one sees "corrective labor camps," the other "slave labor camps;" one sees "rugged individualism," the other sees "exploitive capitalism;" and so on. Compare, for example, reports of identical news events in *Pravda* and in the New York *Times*.

The perception of problems is, perhaps, even more culture-bound, both objectively and subjectively, than the perception of data. Science develops according to the questions people put to it, and these questions in turn depend on their culture. In the history of mathematics this relationship is fairly clear. When land measurement was an urgent problem, geometry was the direction which mathematics took. When navigation required the ability to triangulate, mathematics veered toward trigonometry. When the modern world began to quicken its pace, it asked mathematics for measures of rates of change; Newton and Leibnitz replied with the calculus. None of these inventions could, to be sure, have been made without the necessary prerequisites. But neither would they have been made if men had not asked certain questions. Railroad economics does not evolve until railroad transportation poses problems; business cycle theory does not appear until business cycles force us to ask for it. Theoretical chemists tell us that a whole system could have been built up around nitrogen instead of oxygen if men had asked different questions. There is no predestined plan for the evolution of a science.

The questions men do *not* ask are also determined by their culture. There are certain problems which people immersed in their own culture never conceive. Their language is capable of making perception possible, but it never occurs to them that such problems exist or are amenable to scientific treatment. It takes a cultural outsider to call such problems to their attention. For

> The first requisite [as Veblen tells us] for constructive work in modern science . . . is a skeptical frame of mind. The enterprising skeptic alone can be counted on to further the increase of knowledge in any substantial fashion. . . . Much good and serviceable workmanship of a workaday character goes into the grand total of modern scientific achievement; but that pioneering and engineering work of guidance, design, and theoretical correlation . . . presupposes a degree of exemption from hard-and-fast preconceptions, a skeptical animus, *Unbefangenheit,* release from the dead hand of conventional finality.

The believer cannot see problems in areas where he is a believer. It is the skeptic who sees them; he is an outsider and can view cultural phenomena more objectively. The astonishing record of Jews in science illustrates this point. It is not the Jews who remain within their own cultural setting who make the great contributions; they are as culture-bound as non-Jews. It is only, says Veblen,

> when the gifted Jew escapes from the cultural environment created and fed by the particular genius of his own people, only when he falls into the alien lines of gentile inquiry and becomes a naturalized, though hyphenate, citizen in the gentile republic of learning, that he comes into his own as a creative leader in the world's intellectual enterprise. It is by loss of allegiance to the people of his origin, that he finds himself in the vanguard of modern inquiry.

He is released from the cultural strait jacket of his own background, but he is not immersed in the outside culture either. He is in a uniquely culture-loose position.

> By consequence he is in a peculiar degree exposed to the unmediated facts of the current situation; and in a peculiar degree, therefore, he takes his orientation from the run of the facts as he finds them, rather than from the traditional interpretation of analogous facts in the past. In short, he is a skeptic by force of circumstances over which he has no control. Which comes to saying that he is in line to become a guide and leader of men in that intellectual enterprise out of which comes the increase and diffusion of knowledge among men.

It is not a matter of mere chance that the great revolutionary ideas of our time—Freudianism, Marxism, and Einstein relativity—were promulgated by Jews.

II

Our emphasis so far has been primarily on what some psychologists have called scientific inquiry or problemization as contrasted with the more technical aspects of science known as scientific method. And the conclusion seems inescapable that this phase of science—scientific inquiry—can never be independent of its cultural matrix, which calls attention to some kinds of problems and ignores others. Nor, as some psychologists and neurologists have pointed out, can it be independent of the indi-

vidual scientist's own values, biases, and general orientation.

Must we conclude from this that the much-vaunted objectivity of science was but an illusion of the nineteenth century and the first years of the twentieth? That science gives us no more substantial "truth" than any other approach to knowledge? That it is all wholly relative? Not necessarily. Scientific inquiry may be determined by cultural forces, but the way in which scientific inquiries are pursued, the application of scientific techniques, can and do transcend cultural and other limiting biases. The objectivity of science inheres in "the use of accepted rules of empirical research *after* the problem, the variables, and the experimental design have been decided upon. Here the scientific investigator takes every precaution he can to see that he does not misinterpret what he observes by allowing any subjective bias to enter into the actual conduct of the experiment itself."

Scientific objectivity is achieved not in the dictionary sense of "emphasizing the nature of reality as it is apart from self-consciousness, treating events or phenomena as external rather than as affected by one's reflections or feelings," but in the sense of standardizing the observing and reacting organism. It is not, perhaps, accidental that the first great strides in scientific method were made in sciences whose elementary data—such as heat, gravity, electricity, light—produce fairly standard responses in all human beings. It is true that today scientific knowledge is conceived of as tentative. It is the best to date; tomorrow it may be better. But this is not the same as implying that it is not objective or that it is wholly relative. Objectivity in science is not based on any necessary preconceptions about a fixed and absolute outside world.

It is conceivable, if one wants to hold to such a philosophy, that man can never know anything except his own sensations and reactions. In a certain not-too-metaphysical sense, everyone's sensations constitute "truth" for him, no matter how distorted they may be. Certain impairments of the auditory mechanism, for example, create ringing sensations. The ringing may be "true" for the victim, just as visual delusions may be "true" for persons affected with defects in the visual apparatus. We also speak of religious "truth," which is the subjective conviction, or emotional experience, which the religious devotee has. Obviously, scientific truth is not of this subjective nature. Fortunately one's philosophy of knowledge is not involved in the objectivity of scientific knowledge. For the objectivity of science evolves, not from any point-for-point subjective correspondence with the "outside" world, but with statistical common sense in the literal sense: the common sense of a standardized human being, the common reactions, that is, of a standardized physical organism. (Scientific techniques do not coerce this common response by requiring the acceptance on faith of any first principles or premises. Any system which demands acceptance at the beginning can, by strict logic, arrive at any preconceived conclusion, as theologies do. If you accept A, then you must accept B. In the physical sciences you have to accept A because your reflexes are set up that way.) When these standardized physical organisms react in an identical manner, whether they want to or not, we assume that our inference about the "outside" world is "true." In this sense it is "absolutely" true, the absoluteness resting not on anything outside but in the identity of response in the standardized human organisms. Beginning with these primitive responses of a standardized organism, science develops until sometimes the standardized response consists simply in reading an identical figure on a dial. It is the function of scientific technique to find ways to secure the standardized responses that constitute scientific "proof" regardless of cultural and personal biases.

Scientists are not, of course, fooled by the fact that whole communities of human beings can share delusions and illusions and thus react in a standardized way. We know that under the influence of suggestion whole groups will report hearing and seeing things which, demonstrably, never occur. An audience, for example, will raise its hands, one by one, reporting the first whiff of an odor from a bottle labeled "ammonia" even though it contains only distilled water. It is precisely in order to avoid this kind of spurious "test" of his data that the scientist demands that the conditions of observation or reaction be as carefully protected as possible. The laboratory provides an environment where all kinds of idiosyncracies and suggestibilities are minimized. Equally important are the scientist's efforts to substitute instruments for human sensory equipment in observing and recording data. The scientist's job from the technical point of view is increasingly to design the observing instrument and then to read and interpret the records it produces.

It is undeniably true that the observation of data in the physical sciences can be rendered scientific more readily than observation in the social sciences, that social science techniques suffer somewhat in comparison with the physical sciences in transcending cultural boundaries and individual personal biases. The reason for this lies in the fact that observation in the physical sciences is based on a fairly standardized complement of sensory equip-

ment. Human beings everywhere have pretty much the same kind of physical mechanisms for vision, audition, olfaction, gustation, and kinesthesia. Those who are below normal or below a certain standard in acuity can often be brought up to standard by means of lenses or other aids. Thus the instruments available for the physical sciences depend more closely on man's standardized—or at least standardizable—physical equipment than do those available for the social sciences. Color, sound, electric stimuli, chemical processes, are ultimately responded to by human physical equipment which is fairly standard the world over. Thus reactions to these kinds of stimuli can be fairly easily checked and tested by reference to other standardized reacting organisms, for they are independent of human will. They are under the control of the autonomic nervous system. The normal human retina, regardless of class or culture or individual bias, responds to certain physical stimuli in a more or less stereotyped way. Whether or not you want the pupil to contract in response to light, the smooth muscles will give that result. Whether or not you want to admit that heat has been generated, your reflexes show that it has. You may claim that the temperature is cold, your sweat glands belie your words. You can deny gravity, but you fall down nevertheless. Such phenomena as light and heat and gravity can be "proved" in spite of one's class, cultural, or personal biases. In short, objective "truth" is easier in the physical sciences because it can be based on the autonomic, nonvoluntary responses of a standardized human being. The physical sciences in their elementary and fundamental phases deal with phenomena with respect to which our physical make-up predisposes consensus. Our physical responses to the standardized stimuli science creates for us in the laboratory are not matters of choice.

In the social sciences it is somewhat more difficult to secure such results because we are dealing with phases of the reacting organism that are not so standardized. It is not nearly so easy, as we have already seen, to be sure that we all see or hear or perceive the same things in common stimuli. Here cultural biases and past experiences come sharply into play. Nevertheless, the creation of instruments, tests, sampling procedures, interviewing techniques, systems of recording, all the precautions for assuring accuracy, precision, for minimizing bias, for testing, for validating, for checking—the whole armory of scientific method—are now becoming available for social science data and standardizing our observations anywhere that agreed-upon questions are being asked. The results of personality tests can be tested and checked as accurately and logically as results of thermometers, ammeters, and other instruments. The UNESCO study in international tensions showed that social science techniques developed in the United States could be taught to individuals in other cultures. It is possible to lay out a technical research plan in which all cultures can participate and secure identical results. The techniques of science are independent of the cultural background of their wielders. Everyone everywhere using scientific techniques even in the social science realm gets the same results. It is possible by these means to accumulate data in the social science fields as objective and irrefutable, and as culturally transcendent, as the data of the physical sciences.

Scientific techniques, both physical and social, then, contain their own correctives for class, cultural, or any other kind of bias. Rich man, poor man, beggerman, thief; doctor, lawyer, merchant, chief; Zuñi, Dobuan, Eskimo—all, using scientific techniques, get the same results if they ask the same questions, regardless of their class, cultural, or personal predilections. In this sense it is possible for science, even the social sciences, to achieve supracultural objectivity, to pull itself up by its bootstraps. The use of scientific techniques can correct for the class and cultural and individual screenings of phenomena which permit only certain data to come through and exclude others. In this sense science is objective, beyond culture.

SCIENCE ON THE MARCH

REVOLUTION IN DIESELS

RAILROADS all over the United States, faced with rising costs, are searching for ways and means to economize without sacrificing the high quality of their service to both passenger and freight customers. One already proved way of cutting operating costs is the substitution of Diesel for steam locomotives. It is little wonder that American railroads, caught between rising labor and other costs, on the one hand, and fixed or controlled rates for services on the other, are going ahead at a rapid pace in the dieselization of their motive power.

This revolution has been proceeding without much fanfare. Yet the available statistical data show the high degree it has already reached. The first year in which the number of new Diesels put into service exceeded the number of new steam locomotives was 1939. Since then the number of Diesels installed annually has never been exceeded by the number of steam installations. In 1939 there were 205 Diesels put into operation and only 76 steam; in 1949, 2,800 Diesels and 57 steam (see Table 1).

TABLE 1
CLASS I RAILWAYS

Year	New Locomotive Installations*	
	Steam	Diesel
1939	76	205
1940	120	281
1941	157	469
1942	276	412
1943	438	438
1944	326	918
1945	115	786
1946	86	624
1947	69	1,328
1948	86	2,254
1949	57	2,800

* Source: Association of American Railroads, Bureau of Railway Economics.

It should be noted that the figures for Diesel locomotives are for units and do not represent complete locomotives. In road train service two or more Diesels are usually combined to provide the necessary motive power; in yard switching service usually only one unit is operated. Nevertheless, the tremendous growth in the use of Diesels is well portrayed by the figures in Table 1.

Some of the largest manufacturers of locomotives have suspended the production of steam locomotives. For example, in 1949 neither American Locomotive Company nor Baldwin Locomotive Company received any domestic orders for new steam locomotives, and American Locomotive has announced that it is no longer producing them. The only domestic orders were placed by the Norfolk & Western Railway with its own shops, and the total involved was thirteen.

The most powerful stimulus to dieselization has been the economies achieved with this type of power as compared with steam. Locomotive repairs, fuel, and enginehouse expense are the three operating expenses that may be materially reduced. Furthermore, investment in Diesels can be more fully utilized in that time spent in putting water in boilers, cleaning flues, and in general maintenance and repairs is considerably reduced or completely eliminated. But savings go far beyond these immediate cost reductions. Efficiency is increased through better labor conditions for engineers and firemen, better vision (thus resulting in fewer accidents), and reduction in smoke-control expense. Depreciation charges for the new locomotives are larger compared with the charges for steam locomotives already in use—but not compared with the charges for new steam locomotives.

In order to determine the actual economies that the use of Diesels makes possible, a study was made of one railroad's replacement of five steam locomotives with three Diesels in its switching and transfer service (Table 2). Although the study is based upon a very small sample, the results are generally applicable to all railroads, as is evidenced by the widespread adoption and continued expansion of Diesel installations.

The replacement of five steam locomotives with three Diesels was possible because of the greater utilization of the latter resulting from the elimination of many time-consuming functions. Figure 1 gives the time pattern for the five steam locomotives versus that for the three Diesels.

Whereas, after every assignment, steam locomotives were returned to the enginehouse for coal and water, Diesels required no water and needed

TABLE 2

COMPARISON OF ACTUAL STEAM VS. ESTIMATED DIESEL ANNUAL OPERATING COSTS

(In Terms of Relatives Based upon 1947 Costs)

Costs	Steam: each cost as a percentage of total steam costs	Diesel: each cost as a percentage of comparable steam cost
Locomotive repairs .	15.76	30.16
Conductors' and brakemen's salaries	31.44	100.00
Enginemen's salaries	19.76	100.00
Fuel	19.99	28.90
Water	.75	0
Lubricants	.48	122.36
Other supplies	2.50	100.00
Enginehouse expense	9.32	8.51
Total	100.00	65.70

refueling only every fourth day. The preliminary study estimated that one gallon of fuel oil would be equivalent to 118 pounds of coal. The estimated reduction in operating cost was 34.3 per cent.

A comparison of the actual economies achieved is particularly interesting in view of the economies that were forecast. Since steam locomotives were in operation at the same time as the Diesels, a comparison could be made between actual steam and actual Diesel costs (Table 3).

Instead of 118 pounds of coal being equivalent to one gallon of fuel oil, it was found that 117 pounds of coal equaled one gallon of oil. The Diesel operating cost represented an actual reduction in cost of 40.24 per cent versus an estimated 34.30 per cent. Economies over and above those expected were realized in labor costs, largely as a result of the more efficient utilization of equipment. Fuel savings were greater than anticipated, as might be expected from the continued decreasing cost of oil as compared to coal. The cost of lubricants and repairs was larger than forecast, probably because of the heavy use to which the switchers were put. The study shows without any question, however, that dieselization of motive power results in substantial savings. The cost of the new Diesel installations on the road studied will be recovered in less than three years through reductions in operating costs.

Although this study was quite limited in scope, the results appear to be similar to those accomplished by other railroads. The Baltimore & Ohio Railroad Company completed in 1949 the dieselization of all through freight trains between Glenwood, Riker, and New Castle, Pennsylvania. An indicated saving of $2,000,000 resulted from this change. On the basis of the 1949 operating results, the cost of the locomotives involved is expected to be returned in savings in less than two years. These results are even more favorable than those obtained in our study of Diesel switching engines. The B&O realized a saving estimated at almost $10,000,000 in 1949 from the Diesels it had in service during the year.

The actual savings that result from dieselization will vary with relative fuel costs, weather conditions, track grades, and use, but on many railroads the installation of this new type of power is paying for itself in two to three years. With coal expected to rise in cost $0.25–$0.35 per ton on the basis of the recent wage and insurance fund increase, the fuel advantage of Diesels becomes even greater.

This motive power change away from steam is certain to have far-flung ramifications. The railroads have been the nation's leading coal consumers, and, although there are 29,100 steam locomotives still in operation, their number is being rapidly reduced. The economies of oil over coal are so great that even those roads that derive most of their revenue from the transportation of coal are changing their fuel. The Chesapeake &

TABLE 3

COMPARISON OF ACTUAL STEAM VS. ACTUAL DIESEL ANNUAL OPERATING COSTS

(In Terms of Relatives Based upon 1949 Costs)

Costs	Steam: each cost as a percentage of total steam costs	Diesel: each cost as a percentage of comparable steam cost
Locomotive repairs .	14.31	42.10
Conductors' and brakemen's salaries	33.55	83.28
Enginemen's salaries	21.69	83.27
Fuel	18.38	16.33
Water	.86	0
Lubricants	.51	282.00
Other supplies	2.67	100.00
Enginehouse expense	8.03	7.49
Total	100.00	59.76

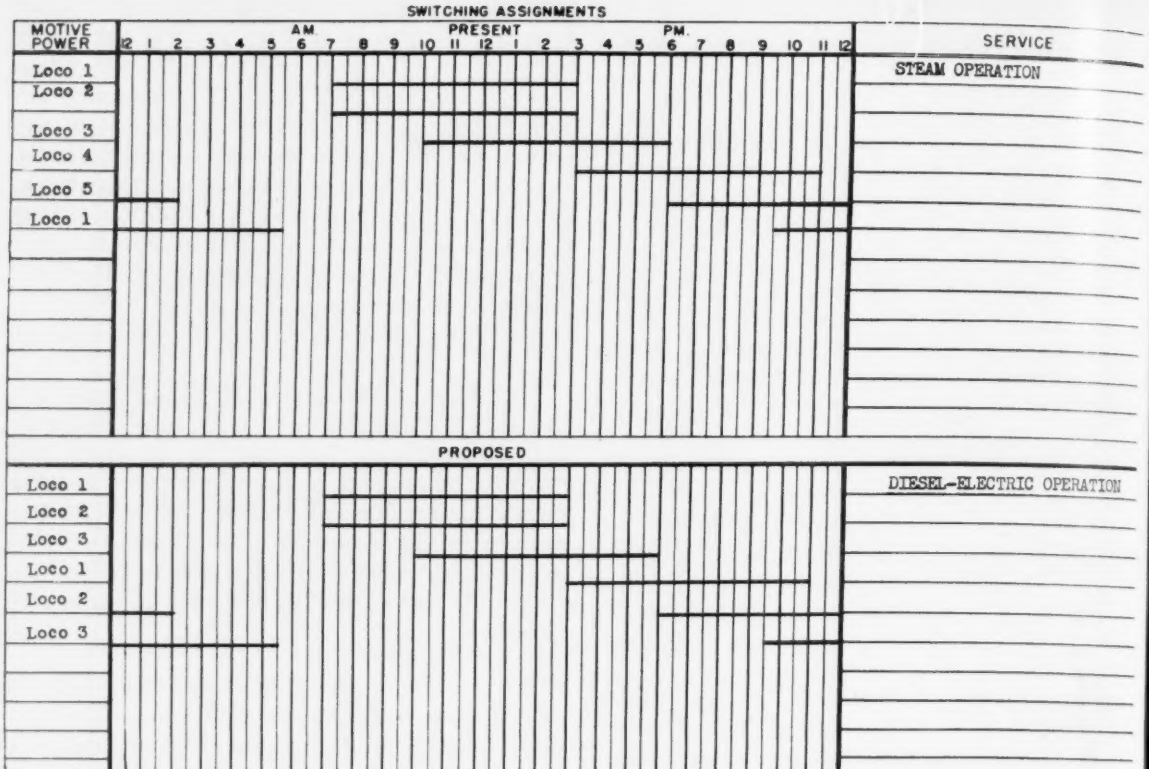

FIGURE 1.

Ohio Railway under ordinary conditions obtains 80 per cent of its revenue from hauling bituminous coal, but it is now purchasing Diesels. In 1949, the Pennsylvania Railroad, largest coal-hauling road in the United States, purchased 300 Diesels. Hope for more profitable railroad operation is accompanying the revolution in Diesels.

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BARBADOS, AN AGRICULTURAL TROPICAL ISLAND WITHOUT WEEDS

BARBADOS, BRITISH WEST INDIES, the easternmost of the Lesser Antilles, is a tropical spot where weeds have no chance. Primarily an agricultural island, all its arable land is under cultivation. Sugar cane, the principal crop, is grown in practically all plantations and in nearly all peasant holdings, with food crops and cotton between two successive crops of cane. About one third the total acreage each year is cane newly planted in November, and the remaining two thirds are grown from stubble. Usually two ratoon crops are obtained, though in better lands as many as four crops can be raised profitably. Soil that is too shallow for sugar cane is under sour grass for stock and mulching.

A typical sugar-cane crop cycle is as follows:

Planted November, say, 1950
First harvest, January to February 1952
First ratoons, " " 1953
Second ratoons, " " 1954

The harvesting season lasts for 3–3.5 months and is usually completed by the end of May. Where the crop cycle has been completed—that is, where the last ratoon crop has been harvested—the old cane stumps are removed by forks, and the field is cultivated with chisel plows. Usually the field will be replanted to sugar cane in November of the same year. The period between the reaping and the replanting of the cane is used for planting such catch crops as maize, sweet potatoes, yams, and eddoes. Although yams and eddoes are not harvested until the year following, cane is interplanted with them.

Carington Sugar Estate in Barbados

In some cases the cane is not replanted until November of the succeeding year. Such land is said to be "thrown out" and is available for the late fall planting (September to November) of sweet potatoes, or of long-term crops such as cassava and cotton. But at no time is the land allowed to lie idle for even a short season, and by law each estate must, by rotation, devote a given percentage of the land to subsistence crops.

Weeding is carried on entirely by hand. The care of a number of acres, depending upon the weediness of the site, is assigned to a laborer, usually a woman. Her sole responsibility is to keep weeds down. And she usually does. Whether the site is planted to sugar cane or to catch crops or to cotton, the worker practices absolutely clean farming, and mulches at the end of the rainy season. Cane thrash piled in heaps at the end of the crop, sour grass (*Andropogon pertussus* (L) Willd.), from near-by plots, and tops of Khus-khus (*Vetiveria zizanioides* (L) Nash), which is planted in rows at the edges of the fields, are used for mulching.

Under these conditions the weeds are never allowed to seed, and each new weed crop must, of necessity, arise from immigrants. Such practices keep under control even the noxious nut grass (*Cyperus rotundus* L.), as well as devil's grass (*Cynodon dactylon* (L) Pers.), which is so persistent in the cane lands of the tropics. The broad-leaved weeds, so commonly present in moist, fertile lands, are easily eliminated as soon as they appear. Barbados is no paradise for weeds.

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BOOK REVIEWS

COLLECTED PAPERS

Reflections of a Physicist. Percy Williams Bridgman. xii + 392 pp. $5.00. Philosophical Library, New York. 1950.

Out of My Later Years. Albert Einstein. vii + 282 pp. $4.75. Philosophical Library, New York. 1950.

THE only fault of Bridgman's book is the colorless title, which fails altogether to imply that this is a collection of lectures and essays—some scientific, some philosophical, some political—which are not by just "a physicist," but are the work of one of the most outstanding physicists and one of the most acute and penetrating minds of our generation. Every physicist should read all of it; every citizen ought to read the political essays; every student of English should study its rich vocabulary and its examples of pungent and incisive writing. Bridgman's specialty has been the physics of matter under high pressure: it is a compliment that, except from one passage, one could not possibly guess from this book the field of physics in which his specialized work has lain.

There are twenty-two items, and a full listing of even the titles would take too much space. Several are devoted to Bridgman's "operational analysis," for which he is noted among physicists and probably (I hope) among philosophers. I quote one of his descriptions of it: "I . . . seek the meaning of our statements and concepts by trying to analyze what it is that we do when we are confronted with any concrete physical situation to which we attempt to apply the concept or about which we make the statement." Excellent samples of his keen analysis of current concepts are to be found in the papers entitled "Statistical Mechanics and the Second Law of Thermodynamics" and "On the Nature and the Limitations of Cosmical Inquiries." Every field of science would be the better for such a scrutiny.

As to what I have called Bridgman's "political" papers (he may not approve of the adjective), they are largely devoted to the situation of the intellectual and professional classes in this era of inflation and of the glorification of the "common man." Instead of using the rest of my allotment of words for comments of my own, I prefer to quote what might well be called Bridgman's law of history:

> Three stages of evolution can be recognized in the present philosophy of the relation of the individual to society. The first granted the right of the bright people to exploit the stupid; the second recognized the right of everyone to receive from society a reward proportional to his contribution to society; and the third and present stage recognizes the right of the stupid people to exploit the bright ones. It is perhaps obvious that my sympathies are with the second, or the median, of these three philosophies.

The Einstein book is likewise a collection, but of a much more random nature. The scientific papers cover some eighty pages; among them is a thirty-page essay entitled "Physics and Reality" and a new, neat, and brief derivation of the equivalence of mass and energy. Scientists will also read with benefit and pleasure the brief memorials of Nernst, Ehrenfest, Langevin, Planck, and Marie Curie, with personal reminiscences based upon close friendship. The other items average barely four pages in length, and some are mere paragraphs composed as messages to various groups on special occasions. They are classified under the headings "Convictions and Beliefs," "Public Affairs," "Science and Life," and "My People." I imagine that they will be read by posterity for the light which they shed on the feelings of a noble, generous, and warmhearted man who by no means confined his interests to the field of his unique achievements. In economics it is not evident that Einstein has read anything but the strict socialist doctrine, which he expounds as though it were a law of nature. Nevertheless, he also says, though in another place:

> We must keep clearly before us . . . what we owe to that freedom which our ancestors have won for us after hard struggles. Without such freedom there would have been no Shakespeare, no Goethe, no Newton, no Faraday, no Pasteur and no Lister. There would be no comfortable houses for the mass of the people, no railway, no wireless, no protection against epidemics, no cheap books, no culture and no enjoyment for art for all.

Not a bad showing for capitalism! Einstein is also aware that the problem of combining socialism and freedom would not be an easy one to solve. Much of the text appears to have been translated; the translation is usually good but not quite flawless, and in one important place (passage in quotes on page 55) says something quite different from what Einstein probably intended.

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WOOD UTILIZATION

Forest Products: Their Sources, Production, and Utilization. A. J. Panshin, E. S. Harrar, W. J. Baker, and P. B. Proctor. ix + 549 pp. Illus. $6.00. McGraw-Hill, New York. 1950.

THE latest addition to McGraw-Hill's splendid "American Forestry Series," now numbering 20 titles, *Forest Products* is primarily an introductory textbook. But it is more than that. It is a useful reference work of technical information on the utilization and manufacture of wood in all its myriad forms.

Some of the products derived from trees, such as fuel wood and round timbers, are among the oldest uses

of raw material known to civilization. Other uses of wood, as for example in the plastics industry and in the form of cellulose in the manufacture of rayon and film, are basic to modern technology.

When, during Germany's preparation for World War II, wood was designated as *Universalrohstoff*, the raw material capable of producing everything, and Goering was named *Reichforstmeister* to control forest management and wood production, the Third Reich realized the indispensability of forest products in its bid for world supremacy. But the United States, which could never have reached its present economic and technological stature without the abundant timber resources with which the nation has been blessed, still takes its forests and its forest products industries pretty much for granted. Quite apart from its value as a text, this book is an important addition to science literature because it provides up-to-date information on the scope of the forest industries and the diversity of their products and, at the same time, shows their relation to, and dependence on, the resource that supports them.

There are three major kinds of forest products, and the authors have logically compiled their book so as to devote a part of it to each group. In the first part they describe the methods of conversion of wood products, including round and mine timbers, railroad ties, furniture, veneers and plywood, containers, and numerous others. In the second part are described the chemically derived products, including pulp and paper, thermal and sound insulating materials, cellulose filaments and film, wood distillation, and wood saccharification. The third part is devoted to derived and miscellaneous products, including naval stores, maple sirup and sugar, tannins and dyes, wood in the plastics industry, and minor products such as oils and oleoresins.

Although the authors avoid describing complex and detailed phases of manufacture, sufficient technical information is given to make the processes of production comprehensible. The numerous illustrations have been selected with care, as have been the bibliographies following each chapter.

In short, this appears to be a highly successful collaboration, perhaps for the reason that the four authors have all had considerable research as well as teaching experience in this complex subject.

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ORIENTAL CULTURE

The Family Revolution in Modern China. Marion J. Levy, Jr. xvi + 390 pp. $6.00. Harvard University Press, Cambridge. 1949.

DR. LEVY has drawn his information from three sources: published works in European and Chinese languages; informants in the United States and personal observation during a year's stay in China from 1943 to 1944 as a member of the U. S. Navy; and material "derived from analysis" (preface). The body of the book is also divided into three parts: the first a statement of the theoretical framework, the second an application of this framework to the "traditional" Chinese family, and the final part to "transitional" China.

Although Levy considers this study "as a bare beginning," he also asserts that most of the written sources, "insofar as they are scientific analyses, lack definitive character" (p. xi). Unfortunately, the elaboration of his theoretical structure is marked by far less concern with the facts than would seem desirable. At the outset Levy is sure of his own "scientific attitude" and is equally sure of its absence on the part of most other scientists, Western and Chinese. However, a "scientific" attitude would seem to consist, for him, of reviving outmoded points of view and then, with the flourish of discovery, refuting them. Thus, in discussing the "economic allocation in kinship structure," he says:

> "Despite the romantic tales of travelers and some social scientists, the income of the family is never simply dumped in a heap . . . and consumed willy-nilly. Such ideas gained a firm hold on the imagination of Westerners . . . of the 18th and 19th centuries and they recrudesce even now from time to time" (pp. 26–7).

If this statement had been made twenty-five years ago it might have had some meaning. It would be interesting to know whether Levy can name a single present-day anthropological work which maintains this. Or, again, he speaks of Sinological research as a "tricky matter" because the Chinese confuse the peasant and gentry pattern. "*Any casual investigation*" of such a problem as remarriage of widows will "bring out the fact" that it "was forbidden in 'traditional' China, if not today," but "*closer investigation and careful questioning*" reveal, however, that "actually the remarriage of widows among the peasants was not only a frequent occurrence but that little or nothing was made of it among the peasants" (p. 46, italics mine). Levy is, of course, to be congratulated on having carried on more than casual investigation, but the facts in this connection are well known. If Dr. Levy had studied Chinese life on the local level, with any degree of control of modern field techniques, he would have found that widows do marry as a matter of course. On the other hand, in spite of his claim to "closer investigation," he did not realize that remarriage of widows during most of, and before, the Sung period was *not considered bad by any level of Chinese society.*

We may now consider Dr. Levy's scientific procedure. Though he lists over two hundred titles in English and Chinese in his bibliography, only five are Chinese. Even as concerns the materials he controls, he fails to make full use of them and rarely discriminates among the relative values of different sources. Many sources that are of obvious relevance and importance are not even mentioned. For example, in his discussion of incest Levy uncritically accepts the conclusions of R. Fortune and then contents himself by saying that he also consulted other "anthropologists" on this subject (p. 21). But, had he looked into Linton's statement on the Nayar, or Slotkin's paper on ancient Iran, or Coon's work on the Arabs, or even a paper of the reviewer, which he listed in his bibliography, he would perhaps have realized better the inadequacy of his working hypothesis.

In Levy's discussion on Chinese family size (pp. 51–55) a similar difficulty prevails. That the actual average family size for all China in modern times is five is borne out by all field investigations, but the assertion that it was never much larger has never been proved. Levy, following Miss Lang, on whom he relies heavily, while sparing no words in castigating the worthlessness of Chinese government statistics in historical times, nonetheless, as soon as there is a preferred conclusion which these "worthless" statistics seem to support, does not hesitate to make use of them.

Given such attitude and procedure, it is not strange that Levy mixes fact with fancy to a very high degree. The factual errors are so numerous that only a few can be pointed out here. For example, although Levy continually brands other authors as mistaking the "gentry" pattern for all China, he himself fails to realize that his formalistic picture of the Chinese clan (pp. 48–51) has a very limited geographical application. The same limitation is true of his statements on land tenure, in spite of his listing of J. L. Buck's survey (pp. 318–20). In his statements on the stages of "absolute age groups" his errors are most unusual (pp. 66–133). He says: "In the classification of absolute age groups, *we shall use terms which were widely used in the 'traditional' period* and are in use today" (p. 66, italics mine). Levy then describes five successive age groups, beginning with "Ying-erh *Shih-ch'i*" and ending with "Lao-nien *Shih-ch'i*." It must be pointed out that, although the first halves of Levy's terms have appeared in Chinese life in one way or another, to the reviewer's best knowledge, the second halves of all his terms adopted here (*Shih-ch'i*) had *never* been used in this connection anywhere either in literature or in speech in traditional China. They are new terms created after contact with the West. Even today the bulk of China's population, which would be classified by Levy as "traditional" China, does not use these terms.

In Levy's discussion of the family revolution in modern China (pp. 273–366), which makes up the entire Part III of the book, his "facts" are, apart from a few incidental references, totally deductively arrived at. He is not unaware of the dangers of this procedure, but he claims to have eliminated them by having subjected his "facts" and theories to "thorough outside criticism" (p. xiv). Unfortunately, even after "thorough outside criticism," Levy's picture of "traditional China" is still far from being borne out by *known* facts. For example, by deduction and the uncritical acceptance of the work of a missionary teacher, it may seem natural to Levy that "there is nothing unusual in the apparently increasing rates of divorce, suicide, juvenile delinquency, illegitimacy, and the like in China" because "it is a familiar story in the West" (p. 377). But what happened in the West may not happen in China. Any student of man knows that cultural differences affect the development of every aspect of a society from industry to politics, family patterns to criminal behavior. Take the case of juvenile delinquency, for example. In the first place, we do not have the slightest evidence whatever that it has been on the increase, because we do not have any statistics for traditional China in this regard. In the second place, there is strong evidence that, where the figures are available, the pictures of juvenile delinquency in contemporary China and the United States are drastically different. For one thing, American juveniles seem to commit more crimes per age group than adults over twenty-one, whereas Chinese juveniles per age group seem to commit fewer crimes than adults over twenty-one. Indisputable evidence for these and other contrasts is readily available in Chinese and in English. But instead of the actual facts Levy prefers deduced "facts."

The reviewer does not wish to give the impression that conclusions drawn by deduction are necessarily false. Science progresses only if there are theories to string the facts together. Facts without theory lead to stultification; but theory without facts is mere speculation. There is no justification for Levy to mislead his readers with so much of the latter, especially since the actual facts are accessible.

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VIRTUOSO

Isaac Newton. E. N. da C. Andrade. 111 pp. Illus. $1.75. Chanticleer Press, New York. 1950.

FROM time to time in the history of mankind," says the author, "there arises a man whose work, whose viewpoint, changes the current of human thought so that all that comes after him bears evidence of his spirit. Such a pioneer, such a leader was Newton." Thus Dr. Andrade in his "Personal Portrait" pays his tribute to "One of Those Who Know" in his gallery of boyhood heroes.

He does not follow a chronological order in his chapter divisions but, after the prologue, considers Newton's scientific status in the esteem of those qualified to evaluate his achievements. Then comes a chapter on the two years of his seclusion at Woolsthorpe, enforced by the ravages of the great plague during the years 1665–67. These years Andrade considers the most creative of Newton's total of eighty-four years. Chapter four depicts the state of science in 1665 as a background against which to project the "Great Discoveries" of the biographee; chapter five has to do with his work on optics; chapter six is captioned The System of the World and recounts the production and publication of his three volumes of the *Principia.* The last two chapters present Newton at the Mint and Newton the Man.

As one would properly expect, the biographer, a physicist, places first attention upon Newton's creation of the mathematics of rates and its application, in *Principia,* to the relations of forces and motion. He stresses Newton's distinction between the philosophic "Why" and the operational "How" of mathematical science. This he considers "no mean contribution" to the thinking techniques of his contemporaries and those who came after.

Those moderns who have had any impulse to place Newton in their "scheme of things" very probably have

not thought of him as a great theologian, as an alchemist, or even as a great public servant (i.e., Master of the Mint). The biographer gives one chapter to the last category of his activities but no more than passing note to the other two of Newton's major time investments. What is said concerning his researches in theology and alchemy is found in the section that attempts to present a "picture of the man." Newton's book on the prophecies of the book of Daniel and the biblical Apocalypse, as well as quotations from his writings on the transmutation of metals, prompts the biographer to characterize "Newton [as] . . . one of the strangest and most baffling figures in the history of human thought." He even considers the following much-quoted statement from Newton's old age a riddle. He thinks that the truth here mentioned had a much wider connotation than the limited part of truth comprehended in science. Newton said, "I seem to have been only like a boy, playing on the seashore, and diverting myself, in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me."

However that may be, the reviewer suggests that he who is looking for a brief, entertaining, yet convincingly written, "Personal Portrait" of the father of mathematical physics, packaged in a pocket-sized volume, will not be disappointed in the book under review.

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GUATEMALAN CLASSIC

Popol Vuh: The Sacred Book of the Ancient Quiché Maya. English version by Delia Goetz and Sylvanus G. Morley, from the translation of Adrián Recinos. xix + 267 pp. $3.75. University of Oklahoma Press, Norman. 1950.

THE *Popol Vuh,* beyond question the outstanding literary production of aboriginal America, has been aptly called the "Bible" of the Quiché Maya of the Guatemala Highlands. A chronicle of "the beginning and the origin of all that was done in the town of the Quiché, by the tribes of the Quiché nation," it includes a wealth of information on the cosmogony, religion, customs, and material culture of the people which has unhappily been almost ignored by the student of anthropology.

Compiled by an anonymous Indian scribe in the middle sixteenth century, the *Popol Vuh* was copied and translated into Spanish by Father Francisco Ximenez about 1700. The original was lost; but Father Ximenez' copy and translation, after one hundred and fifty years of oblivion in church archives, were rediscovered and presented to the scholastic world in the mid-nineteenth century by Scherzer and Brasseur de Bourbourg. Since then, numerous editions in Spanish, German, and French have been published in both New and Old World capitals. But, although the original Ximenez copy of the manuscript has for some years reposed in the Ayer Collection at Chicago's Newberry Library, the *Popol Vuh* has not been available to the English reader until publication of the present edition.

Adrián Recinos, for sixteen years Guatemalan minister and ambassador to this country, is one of the outstanding students of the indigenes of his native land. Master of the Quiché tongue, he is also endowed with the literary background and familiarity with the customs and ways of thinking of the Maya necessary to combine scientific accuracy in his translation with preservation of the feeling and spirit of the original. Delia Goetz and the late Dr. S. G. Morley did an admirable job of carrying this success across still another linguistic barrier into English.

Recinos' lengthy introduction provides a scholarly historical background for the book, including a summary of related aboriginal literature, a sketch of the original recorder and translator, Father Ximenez, and a history of previous translations and editions of the book. The anthropologist will find this background study lacking in certain subjects, such as specific definition of the broader linguistic and ethnic relationships of the Quiché, equation of the historical evidence of the chronicles with both the growing body of archaeological information on the Quiché area and the historical and archaeological patterns of Middle America as a whole, or appraisal of the *Popol Vuh's* rich content of information on the behavior and everyday life of the early Highland Maya. Lothrop and, recently, Wauchope have published preliminary attempts to bring together the archaeological and historical evidence in this area. It is to be hoped that this new translation will spur continuance of such work, and will encourage social anthropologists to make use of the many other almost untapped resources to be found in the book.

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PREHISTORIC ENCYCLOPEDIA

A Hundred Years of Archaeology. Glyn E. Daniel. 344 pp. $3.50. Gerald Duckworth, London; Macmillan, New York. 1950.

THIS highly informative book is the latest of ten in "The Hundred Years Series," designed to assess the achievement of various branches of learning since the middle of the nineteenth century. With only a secondary interest in the archaeology of historical times, which he regards as at best a supplement to the written records, the author's chief concern is with prehistoric archaeology, which he treats as technique, accumulation of objects, and the fullest available story of the early life of mankind.

Four introductory chapters review the rise of antiquarianism, its development in connection with the revolution in geological knowledge, the birth of prehistoric archaeology (1840–70), and its coming of age (1870–1900). The author then recounts the evolution of archaeological aims and techniques before 1900 through some fascinating sketches of Mariette, Schliemann, Pitt, Rivers, and Petrie. Equally attractive is his story of the discovery of Near Eastern civilizations (1900–

50), together with a brief presentation of the fundamental characteristics of each. European prehistory is treated with the same method, but owing to its lack of glamor as compared with the prehistory of the Near East, it makes less interesting reading. Then follows a similar survey for India, China, and America. Africa is practically ignored. Of surpassing interest is chapter nine on the refinement of techniques and methods since 1900, for it is here that one sees most vividly the processes, the nature, and the aims of prehistoric archaeology as a mature "humanity," in intimate relation with the culture of the age. The last chapter, Retrospect and Prospect, which is really a study in prehistoriography, is followed by an invaluable "Chronological Table of Main Events: 1840–1940," a select bibliography, and a perfunctory index.

This solid piece of work may not be for the layman, but for the advanced student it is extraordinarily helpful and absorbing. Even the specialist will find in it not merely an authoritative prehistoric encyclopedia, but one planned and executed organically, and enriched with numerous illuminating insights.

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HOW TO KNOW THE BIRDS

Birds of the West. Ernest Sheldon Booth. xi + 402 pp. Illus. $6.00. Stanford University Press, Stanford, Calif. 1950.

IT IS certainly true, as the author suggests in the preface, that, from the standpoint of a book that teaches, many bird books are not all that might be desired. As a consequence "people are still unable to learn the birds." Part of this failure inheres in the many possible variations of approach to the subject, illustrated by such distinctly different works as the superb life histories by A. C. Bent, the artistic representations of Audubon popularized by William Vogt, and the straightforward manuals of identification by Roger Tory Peterson and others. With such diversity of possible things to study about birds, the beginner has to select carefully indeed if progressive learning is to take place.

In the study of birds the first requisite is a ready means of field identification. This is not always easily obtained, because in addition to the problem of selecting a book fitted for this special purpose the beginner is faced with the fact that most of the books on identification are not organized to provide a simple, systematic means for learning the birds.

It is here the Dr. Booth's *Birds of the West* fills a real need. It is organized as a key, similar in nature to botanical keys. This presentation makes it possible for the student to acquire knowledge of orders and families in the process of identifying any bird, and should greatly help to stimulate and maintain the beginner's interest, and speed his learning. With this sort of beginning in bird study, the student is more likely to progress to more difficult identification, the study of habitat relationships, and other facets of bird life.

Following the preface is an explanation of how to use the keys. Thirty pages are devoted to identification of orders and families. Most of the remaining pages provide keys for genera and species found in North America west of the Great Plains from Canada to northern Mexico. For lagniappe, Booth includes an appendix of some twenty pages on such miscellaneous subjects as nests and eggs, bird photography, feeding tables and nesting boxes, bird banding, bird societies, and journals, magazines, and reference books on birds. The book concludes with separate indexes of common and scientific names.

Dr. Booth has avoided all but those details consistent with his objective: to aid the beginner to learn the names of birds. Such a clear-cut objective is admirable, but it sometimes leads to complications. For example, descriptions are restricted to the mature males in full plumage (except in several colored plates); songs and flight habits are rarely mentioned, and distribution given is very general, with only limited information on habitat. The author recognizes these rather unavoidable inadequacies and recommends companion volumes to fill the gaps.

For each species there are included details on construction and location of nests, and number and color of eggs. Subspecies are identified primarily by distribution—a method having certain evident limitations.

Birds of the West is nicely and usefully illustrated by Harry Baerg and Carl Petterson, mostly in black and white but including several color plates. Paper used is of exceptionally high quality, all of which makes this a serviceable and attractive book.

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ORGANICISM

Das Biologische Weltbild. Ludwig von Bertalanffy. 202 pp. Unbound, 11 fr.; bound, 14.50 fr. A. Francke ag. Verlag, Bern. 1949.

THE author presents his method of attack in biology (the organicismic conception) as victoriously taking over the field after two classical contenders, mechanism and vitalism, have worn themselves out fighting each other. These two classical opponents have much common ground, and neither is adequate to the demands of biology.

The organicismic concept offers, in contrast, (1) "wholes" and integrated "systems" for the vanquished "analytic-summative" weapons; (2) dynamism for the static quality of mechanical contrivances; (3) the idea that the primary characteristic of organism is activity rather than mere reactivity. The author emphasizes that his view is not mystical. It remains amenable throughout to the scientific method, accepting the peculiar organization of living entities as facts, different in kind from the facts of the inorganic world, but nonetheless facts. The goal of the organicist is, like that of any scientist, to determine exact laws.

Basic to the organicistic standpoint is Bertalanffy's idea of "open system" in contrast to the familiar "closed system." The living organism is not shut off to the outside world as was the system within the atom for nineteenth-century physics. The organism constantly gives of itself to the outside world, and from the latter takes to itself, maintaining during this ceaseless flux a kind of equilibrium (*Fliessgleichgewicht*), or "steady state"—the condition in open systems analogous to true stationariness in closed systems. The concepts of open system and steady state are not only essential to the understanding of biological phenomena, but have revealed a new world in physics, where their effect on the notion of irreversibility of process in thermodynamics is of the utmost importance.

The author, indeed, by no means limits the relevancy of the organicistic conception to biology or such allied sciences as physiology, psychology, medicine, and sociology. It has repercussions in physics, chemistry, astronomy, and in the universal picture of reality.

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MAN'S ESSENTIAL NATURE

On Being Human. Ashley Montagu. 122 pp. $1.95. Schuman, New York. 1950.

THIS is a fascinating attack by a well-known anthropologist on the Social Darwinism which holds that life is struggle, competition, the survival of the fittest.

If Montagu were simply bringing new evidence and interpretations to bear on the importance of cooperation, it would be one thing. It is something else to hold that "when social behavior is not co-operative, it is diseased" and that "whatever is opposed to love, to goodness, and to co-operation is disharmonic, unviable, unstable and malfunctional—evil."

It is true that competition has been prettified and glorified, and that Malthus and Darwin have been used to justify cheap labor, child labor, unemployed labor, imperialism, and the exploitation of "inferior races." It is also true that the study of cooperative behavior has been neglected. But one wonders if the inevitable conclusion is that all competition is disfunctional for individuals and for groups, or that competition has ever been absent in any society. One may agree that there is "not a shred of evidence that man is born with 'hostile' or 'evil' impulses which must be watched and disciplined." But the nature of social interaction, regardless of type of social structure or brand of culture, is that all cannot have all their needs met all the time. Always and everywhere there are bound to be some frustration, disappointment, resentment. Montagu recognizes this when he cites the Eskimos and the Australian aborigines as examples of cooperative groups in which "social feeling . . . appears to be almost as natural as breathing," and where childhood is characterized by a high degree of permissiveness and a low frequency of frustration. Two questions are suggested here: (1) Are these typical nonliterate societies, and (2) is there a closer relationship between competition and urban, industrial civilizations, whose social solidarity rests largely upon a high division of labor, than there is between competition and the nonliterate society, which is based mainly upon kinship institutions?

Man does want "dependent security, the feeling that one is part of a group, accepted, wanted, loved, and loving." But he has another "nonvital basic need," which Montagu calls "social recognition." He is likely to achieve this recognition in part at least, from some kind of competition, even if it is competition in being cooperative. Many will not quarrel with Montagu, Galt, Burrow, and others that competition in the Western world makes us "the partitive, disordered, hostile, egocentric creatures that we have become." The treatment which Montagu prescribes is "social engineering" through teaching children who are to be the adult members of the next generations. He recognizes that this will require very resourceful teachers who are supported far better than present teachers are by the general citizenry.

Montagu, as always, is a stimulating and excellent writer. This little volume will repay careful reading, and the larger and more technical work along these lines which the author promises in several years will be awaited with much interest.

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Organization of Behavior. D. O. Hebb. xix + 335 pp. Illus. $4.00. Wiley, New York. 1949.

THIS book presents a modified Gestalt theory in which experience is considered to be a prominent variable. The major premise is that psychological phenomena may be described and explained most fruitfully in terms of neural theory. Perception is the key phenomenon. Perceptual integration is an assembly of cortical cells. The assembly grows slowly and because the individual cells have fired at the same time on a number of occasions. The development of a cell assembly involves fractionation and recruitment of individual cells and the occurrence of structural or metabolic changes at their synaptic connections. The multiplication of synaptic knobs is suggested as the basis for the organization of a cell assembly. A closed solid cagework, or three-dimensional lattice, with no regular structure, and with connections possible from any one intersection to any other, is given as an analogy of the cell assembly. The temporal duration of a cell assembly is attention, and the temporal organization of cell assemblies, named "phase sequences," provides for further complexity of perceptual integration. Learning, motivation, emotional disturbances, and intelligence are discussed in terms of this neural theory, in terms of perception, and in terms of each other.

The neural theory is admittedly gross, and probably impossible to test, but its presentation results in provocative discussion. New hypotheses make an appearance. For example, the length of the primary learning

period in different species is roughly proportional to the ratio of the total association cortex to the total sensory cortex; and, hunger is an addiction. It is difficult to arrive at an over-all evaluation, for the quality of the book is uneven. There are frequent side discussions, not immediately relevant to the point at issue. This book will undoubtedly be of greatest interest to those, who with the author, believe that explanation for psychology is in terms of neural function.

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Toward a Science of Human Behavior. Mark A. May. v + 93 pp. Yale University Press, New Haven. 1950.

THE Institute of Human Relations at Yale University, founded twenty-five years ago, might be thought of as an academic prototype of the present United Nations organization in that it brought together the various academic disciplines concerned with a study of human behavior. The institute was founded to develop an interdisciplinary science of man out of the independent sciences of behavior as found in such areas as psychology, psychiatry, sociology, and anthropology.

These efforts to achieve such an interdisciplinary science provide an interesting story of intellectual cooperative efforts by persons from many different fields. For example, during the period of its existence the following departments have participated in the institute's activities: anatomy, anthropology, biochemistry, child development, economics, education, government, history, law, pathology, pediatrics, philosophy, physiology, psychology, religion, and sociology.

The efforts to break down barriers between disciplines, to integrate the biological and medical sciences with the social studies, and to present a new outlook for students of law, medicine, education, and other professions, were met at the time of the founding of the institute with both laudatory and skeptical comments. The lines of Pope

> Know then thyself, presume not God to scan;
> The Proper study of mankind is Man

were frequently cited. One writer commented to the effect that Pope wrote it, Yale believed it, and Rockefeller financed it. This statement is more than just a metaphor of amalgamation; it represents still another type of cooperation.

This book by Professor May, director of the institute for many years, reviews the origin, purpose, and organization of the institute, summarizes its major achievements, and takes a glance at the tasks which remain to be done.

The story is stimulating reading for anyone interested in ideas, their development, and their implementation. The institute's existence is a tribute to Yale.

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LIFE IN HOPILAND

Sun in the Sky. The Hopi Indians of the Arizona Mesa Lands. Walter Collins O'Kane. xvii + 261 pp. $4.00. University of Oklahoma Press, Norman. 1950.

A CHANCE visit with a Hopi Indian grandfather, followed by other visits to Hopiland during the last few years, led Professor O'Kane, an entomologist, to write *Sun in the Sky.* "To be with these people," he notes, "to witness their activities, and to come to know and understand their thoughts and beliefs is a rewarding experience. In this book I hope to share with others some of my experiences, and to convey to others an understanding and appreciation of Hopi life and character."

O'Kane's equipment for his task apparently did not include the ethnologist's orthodox kit of tools—training and experience in anthropological field techniques—nor, happily, did it contain the jargon of the professional social scientist. Evidently it did include a marked capacity for detailed, accurate observation of human behavior, in partial situational context at least, and an active interest in trying to understand and convey the human values and motivations underlying observable behavior, seldom found in the work of social scientists. The result is a sympathetic, readable, though limited, account of the everyday behavior of the Hopis as they live their tradition-rooted but forward-oriented lives on their arid Arizona mesas, within, and yet apart from, modern American civilization.

"In visits with the Hopi people," writes O'Kane, "I have been impressed and baffled by their concept of a spirit world and its relation to the physical world—impressed because their beliefs are so continuously a part of their daily activities, baffled because to a white man their concept in some of its aspects is difficult to grasp." To understand the full implications of this concept, really to think as a Hopi thinks, one ought to have been brought up a Hopi. In contrast to the white man, the Hopi lives in an ever-present spiritual world, and at the core of Hopi thinking the author finds an indissoluble union of the seen and the unseen. For example, the Hopi assumes that what he does with his body he is doing also with his spirit. Prayers embodied in the rituals are not primarily concerned with personal virtues such as industry, frugality, and honesty, which are acquired as a result of closely knit village life in the difficult desert environment; rather, in the ceremonies, the Hopi "ask for those gifts which, in Hopi belief, the spirit world can bestow—safety on a journey, abundant rain for the crops, restoration of health, and long life." But always one must desire and will for a good outcome and keep a "good heart," for a man who thinks evil will work evil, and vice versa.

The author's quest for the meaning behind Hopi activities and their message for our time takes him to some little-known corners of the Hopi world. For example, he gives a vivid account of an arduous expedition to the ancient cave in the Colorado River canyon where the Hopi used to get their salt. The chapters on

Hopi craftwork, agriculture, and trade are first-rate, and there is interesting material on the ceremonies, especially regarding synchronization of chanting, drumming, and gestures.

As might be expected, however, the intricacies of Hopi social organization are dealt with so superficially as to be misleading. Although well-documented research is available on the subject, when missions are discussed no hint is given of the disorganizing effects which certain Christian missionaries and other influences have had on the socioreligious system and personality in some Hopi villages. The weakest part of the book, in this reviewer's opinion, is that relating to the problems of government administration in Hopiland, which are imperfectly understood by the author.

As an easy introduction to Hopi life and values, however, this attractively illustrated volume will be found helpful, especially by laymen and high-school students trying to understand the customs and thought-world of an ancient and at the same time modern Indian tribe. Its usefulness to scientists is impaired by the absence of references citing the exact sources of specific material taken from the extensive Hopi literature, such as one expects in a publication from a university press.

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Hopi Kachina Dolls, with a Key to Their Identification. Harold S. Colton. xv + 144 pp. Illus. $7.50. University of New Mexico Press, Albuquerque. 1949.

ONE of the most interesting religious concepts among American Indians is that of the kachina cult of the Peublo Indians. No vague or nebulous beings are these rulers of the clouds and bringers of rain, but, as represented by initiated, costumed dancers, and carved in painted wooden images, they have definite and concrete characteristics that differentiate each kachina from his fellows and make him immediately recognizable to the tribesmen.

In representing a wide variety of personalities among these nature gods, native artists have exercised a fertile and vivid imagination in giving form and color to their symbolic figures. The Hopi alone conceive of more than two hundred and forty different kachinas. They also recognize, and sometimes reproduce, kachinas belonging to other pueblos. In addition, they have other nature gods, not kachinas but similar to them, which are occasionally represented in carvings. When the kachinas are called and the masked impersonators dance in the Hopi villages, they bring kachina dolls, miniature representations of themselves, as presents for the children, who thus learn to differentiate them.

These are the genuine kachina dolls made since early times by parents and uncles to impress and educate the children. In modern times these dolls have so fascinated collectors because of their artistic merit and variety that to supply tourist demands Hopi carvers are turning out dolls in quantity, made especially for sale. No less intriguing in appearance, these pseudo kachinas are deceiving, since the carver does not feel constrained to follow the traditional conventions that identify the members of the real pantheon. Finally, also catering to the tourist trade, white men have taken to imitating kachina dolls for sale to the uninformed.

In this interesting book, Colton, who is director of the Museum of Northern Arizona at Flagstaff, explains what the kachinas are, the purpose of the kachina dolls, and the method of their manufacture.

The principal body of the book consists of descriptions of two hundred and fifty of the more commonly represented kachinas, and a key to their recognition. Although the author modestly states that the book is for the purpose of assisting collectors in identifying their dolls, there are few, if any, museum curators who cannot profit by its use. The descriptions are accompanied by sketches of kachina masks, and there are sixteen plates, eight of which are in color, from photographs by Jack Breed. The illustrations will be a delight to lovers of native American art, and the book in general will be of the greatest help to ethnologically minded collectors.

In preparing his material, Colton acknowledges invaluable assistance from Edmund Nequatewa, of Second Mesa, and Jim Kewanwytewa, of Oraibi, who checked and augmented his material.

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CORRESPONDENCE

PLUS ÇA CHANGE, PLUS C'EST LA MÊME CHOSE

RECENTLY I chanced upon the vice-presidential address C. A. Young gave at the Buffalo Meetings of the AAAS in 1876. It was entitled "American Astronomy—its History, Present State, Needs and Prospects." Perhaps others who, like myself, have watched with sorrow the roughness of the course of the National Science Foundation, and received with humble gratitude assistance from the ONR, may be interested in the appended historic passages and predictions.

During the first half century of our national existence, science of every kind was utterly neglected; astronomy especially seems to have been regarded almost with aversion by the popular mind, as something aristocratic and unrepublican. When Hassler in 1807 submitted to the Government the project for the Survey of the Coast, it contained a provision for the establishment of an observatory to supply the needed astronomical data; but the proposition received no favor. The original law authorizing the Survey omitted all mention of the subject, and the law of 1832 expressly prohibits any such establishment. In 1825 President J. Q. Adams in his first message to Congress suggested and recommended the founding of a National Observatory, and in very eloquent language pointed out its importance; but the recommendation was received with scorn and ridicule. In referring to the observatories of Europe, he had spoken of them as 'light houses' of the skies, and the expression was seized upon and made for years a byword of reproach. The prohibition of an observatory in connection with the Coast Survey undoubtedly arose from the partisan feeling which had been excited by President Adams' suggestion, and it was not until 1842 that the National Observatory could be established, in a semisurreptitious manner, as a Depot for Charts and Instruments. The same narrow Philistinism is still rampant, though fortunately no longer regnant, in the halls of legislation, and on every opportunity manifests itself in opposing all measures for the encouragement of Art or Science. The debates on the appropriations for the Transit of Venus will be recollected by many as a case in point. . . .

As to the future of American science, perhaps I am sanguine: but I fully believe that our free institutions are favorable to the highest scientific development, and that during the coming century our nation will take her place among the leaders of scientific progress. All signs seem to me to point that way. To be sure, there must first be a great change in the spirit and temper of our students, and in the estimate of purely scientific work by the community at large. But the change appears to have already begun. An increasing number of our young men are willing to take the time needed for thorough training, and prefer the pursuit of truth to the mere getting of money; so that we have growing up among us a crop of young mathematicians who, in ability, will undoubtedly equal their predecessors, while in numbers, culture, incentive and opportunity, they have greatly the advantage. Our Universities are broadening and deepening their scientific and mathematical courses, and providing for post-graduate studies. Some of our men of wealth are finding in science a career and opportunity of honorable distinction more satisfactory than any other. Our public men are more ready to aid us in obtaining reasonable assistance from Government when needed. In short, everything seems to me to indicate progress, which since its beginning in 1836 has been uninterrupted, and is likely to continue. Therefore, I augur well for the future, and am confident that if the record of the century past can be called honorable, that of the century to come is to be glorious.

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THE FAIR

John never quite knew why, year after year,
He went to the county fair. But he did,
And every fall he told Jane he would never
Go again. He cared nothing for the shows;
He didn't give a hoot for the horse-racing;
He hated the noise, the crowds, and the smells;
He wasn't interested in hearing speeches
By half-baked politicians; none of his
Hogs or sheep got to be prize specimens;
He hadn't been on a merry-go-round
Since he was a boy, and he didn't propose
To go on one now. He didn't spend money
On slot machines or the shooting gallery.
But every year he went, all by himself.
"How was it?" Jane would ask when he returned.
"Terrible," he'd say. "Worse 'n 'twas last year—
Saw only three or four people I knew."
Why did he go? Well, he went to funerals,
Didn't he? He didn't like them either.
He went to Decoration Day programs,
Didn't he? And they were pretty sad, too.
He believed a man should do his grim duty
For God and country and, if necessary,
For Henry Samuels' prize Hubbard squash,
The county agent's spiel on seed potatoes,
And Mrs. Peterson's canned Lima beans.

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